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INVOLUNTARY MOTOR REACTION TO PLEASANT AND UN- PLEASANT STIMULI.

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Study of the emotions, in one way or another, has had a conspicuous place in the work carried on in the Harvard Psychological Laboratory. In this field of all others, perhaps, the investigator gains easiest access to the goal of physiological psychology—the determination, namely, of the quantitative and qualitative relations obtaining between those wonderful correlates, mind and body.

That elementary organism, the amœba, when jarred by its environment or more directly stimulated, contracts and tends to take the spherical form. On the other hand, all its movements of self-gratification are processes of expansion or extension. This observable double tendency seems to be the type, almost symbolically expressed, of a deep biologic law which science has shown to exist with almost infinite adaptation to circumstances and habit, in every organism. Experiment proves that, like other impulses of the lower animals, it exists persistently in man.

Professor Münsterberg has advanced the hypothesis that stimuli which cause action of the extensor muscles are as a rule agreeable, while stimuli which cause action of the flexor muscles are as a rule disagreeable. This tendency we should nat-

urally expect to find more fully and simply expressed in the case of the lower animals than in that of the higher. In early organisms such a correlation is necessary to ensure the survival of the organism and the possibility of evolution. For in the lower forms of life there should be such a strict correlation between the agreeable and the advantageous on the one hand, and between the disagreeable and the disadvantageous on the other, that the advantageous would be the agreeable and would be accompanied by expansion and movement towards the stimulating object, while any disadvantageous and consequently disagreeable stimulus would cause contraction and withdrawal from the stimulating object.

This correlation, however, would be more true in animals and savages than in civilized man. In our civilized state we have lost our primitive simplicity. We are still mal-adjusted to many civilized conditions, owing to our change of environment from the savage to the cultured state. By immediate inheritance and habit we have learned to control our motor reactions, to suppress the outward signs of our feelings. We often, perhaps mistakenly, think that even to ourselves as physical organisms, the disagreeable is advantageous and the agreeable is disadvantageous. We often enjoy pain and dislike pleasure. We have a thousand contradictory tendencies that run counter to any such simple rules of motor reactions as that above stated.

Yet in spite of these complications it is plausible that there exists the correlation claimed by Professor Münsterberg, showing itself strongly in the midst of conflicting tendencies. It was to test the validity of this hypothesis that the following research was conducted during the college year 1895-96.

It might be expected that a less educated class of subjects than those we have had would give more marked results, as far as reactions are concerned. Young children or savages would surely show motor reactions more strongly marked to disagreeable or agreeable stimulations. But even in our results, we have found a plain tendency in favor of the theory mentioned.

It must be mentioned that with many subjects we could get no perceptible reactions to the sensory stimuli. Some subjects seemed constitutionally averse to any motor reaction. The

stimulus would generally be pronounced either pleasant or unpleasant, and yet the subject would show no motor reaction whatever. This lack of reaction was very marked in some cases. In a few instances the subject pronounced the stimulus indifferent, yet often reacted to it one way or the other.

Some subjects were very sensitive and seemed to go all to pieces on any disagreeable stimulus, and would show most surprising and seemingly contradictory reactions. These points we will try to bring out fully in our statement of the results.

METHOD OF EXPERIMENT.

The emotional stimuli mostly employed were odors, but sounds and variously colored lights were also used, to a much less extent. It was greatly desired that the stimulus in each case should give an effect as purely painful or pleasurable as possible. Many subjects were employed and the stimuli were given often several times to each, on which accounts odors seemed the most fitting of possible agents. These furnish about the only means, indeed, of causing a constant pleasurable stimulation in the practice of the laboratory. Odors have the further advantages of being unlimited in number and in action independent of the subject's power of imagination. It was much more difficult to find for each subject a positively disagreeable odor than a positively pleasant one, students of chemistry being especially hard to suit with a sufficiently unpleasant smell. Constant care was needed and employed to suit the tastes of the various subjects in this regard, the objects being to employ types of pleasure and of pain.

The particular olfactory stimuli employed were kept in ounce vials on a stand made for them. It is hardly possible to make any classification of them as pleasant or unpleasant. Roughly, however, they may be arranged in the following order of agreeableness to the greater number of the subjects employed, the most pleasant first, but the middle ones varying greatly in this regard. Naturally the most emphatic members of the series were those most used. Oil of bergamot, cologne water, heliotrope, methyl acetate, oil of cloves, tincture of musk, ethyl iodide, spirits of turpentine, xylol, eugenol, oil of eucalyptus,

iodoform, cider vinegar, bisulphide of carbon, ethyl bomeol and camphor, sulphuric ether, toluidin, allyl alcohol, tincture of asafœtida, diamylamine, acetic acid, ammonium valerianate. A few subjects avowed no pain from any of these, and for these ammonia was employed in place of a real odor. It will be noticed that 'disgusts' are not included in our list, associations not being desired in these experiments, but only pure affective tones.

Panes of glass a foot square, colored red, blue, green and orange were used for subjects with color taste highly developed, the panes being held before their eyes. Similarly, for musical subjects, such sweet tones as tuning forks can give were applied as stimuli, with harsh noises for a contrary effect.

The hands and the head were chosen as the bodily parts most suitable for reaction, these being the most sensitive to motor stimuli and the most convenient. According to the theory in question, the hands should relax and the head drop back under agreeable stimulus, while under disagreeable stimulus the head should drop forward and the hands contract.

The mechanical plan employed for the direct registration of the flexion and extension of the head and hands was as follows: The subject was seated in a comfortable arm chair. A tightly-fitting pasteboard cap was placed on the head, from the center of which a strong thread extended over an easy-running pulley to the extremity of the lever of a Marey tambour. Because the antero-posterior movements of the head were sometimes considerable this lever arm was about twenty-five centimetres in length. By a careful centering of the pulley in the circle of head movements, record of the occasional lateral motions of the head was avoided, account of these not being desired. Pneumatic pressure transferred in the usual way the rise and fall of the receiving tambour's arm to the pen of another Marey tambour, writing on smoked paper on a revolving drum.

The apparatus adjusted to the left hand consisted of a bulb small enough to be fairly grasped in the closed fist. It was at first difficult to find a bulb without so much resistance to compression that the subject's constant attention was necessary to keep it in the state of partial compression needed to secure rec-

ord of the extensor movements of the fingers. But at last a bulb made of a soft sponge from which the center had been cut, enclosed in thin rubber dam, was hit upon, and this served as a most sensitive and, indeed, adaptable instrument; for sponges may be found or cut of any desired degree of resilience. The varying pressure of the hand was pneumatically carried to a receiving tambour and recorded on the smoked cylinder at the left of the tracing from the head.

To register the movements of the right hand in states of organic pleasure and displeasure a different form of instrument was employed. About the second and third fingers, as the most sensitive and most powerful, a comfortable ring of brass foil was fastened. This was attached directly to the lever of a tambour and as close to the fulcrum as possible, that all motion might be emphasized; and it was adjusted so that when the fingers were partially flexed the tambour rubber was plane. Comfort of the hand in this case was found important in order to avoid voluntary attention to it and its reactions. The flexor and extensor movements of the two fingers were as before transmitted to a pen tracing on the right of the record of the head.

To secure constant pressure at the start in the three sets of apparatus, the open ends of branches from the three conducting tubes were arranged side by side convenient to the operator, and fitted with clips so as to be simultaneously closed when all was ready and the kymograph in regular motion. The speed of the recording drum was such that one revolution was made in about five minutes. The cylinder was 14 cm. in diameter and 25 cm. long, suitable for two records such as these without change of paper. Straight normals for the better measurement of the curves were regularly run round the drum by stationary pens.

Record of the various conditions of each experiment was written with a stylus on each sheet, including name of subject, temperament, subjective experiences, stimulus, nature of effect whether pleasant or the contrary, date and direction of muscular movement indicated in each reaction. The subjects were mostly Seniors and Juniors of Harvard college and of Radcliffe college and graduate students working in the laboratory.

Their number was nineteen. Inquiries as to emotional likes and dislikes were regularly made and as to musical and 'artistic' education.

RESULTS.

1. *Under Pleasant Stimulation.*

Taking each movement or lack of movement, whether of head or of either hand, as a separate case, we have recorded 500 effects of sensory stimuli which were considered pleasant by the subjects. Of these, 118, or 23%, were cases of flexion of hands or forward movement of the head, 134, or 27%, were cases of no reaction whatever, and 248, or 49.6%, were cases of extension of hands or backward head movement. Considering the cases of actual reaction alone, there occurred 67% of movements of extension and 32% of flexion—a proportion of more than two to one. The tendency under pleasant stimulation is therefore strongly toward extension.

The two hands and the head did not necessarily act together in the same way. The left hand seems much more sensitive and more given to expressive motor reaction than the right, and as our subjects were mostly right handed, it would seem justifiable to infer from this that the right hand is more civilized and more under control and less naïvely expressive than the left. If the idea stated in the beginning is tenable, that civilized man is more likely to inhibit emotional expression than a savage, then we might expect the right hand to be the more inhibited and the less likely to react.

Counting the cases of pleasant stimuli where the left hand showed no reaction, we have for the left hand under stimuli pronounced agreeable results as follows: Flexion 21%, extension 60%, no reaction 19%. Out of 184 stimulations the left hand shows flexion 37 times, extension 112 times, no reaction 35 times. If we compare the left hand with the right, the percentage of 'no reactions' is seen to be much less for the left hand, while that of both flexions and extensions is greater. Under pleasant stimuli the right hand showed, flexion 20%, extension 40%, 'no reaction' 40%. That is, in a total of 130 cases, the right hand flexed 27 times, extended 52 times, showed 'no re-

action' 51 times. The left hand then was indifferent 19% when the right hand was indifferent 40%; it extended 60% where the right hand extended only 40%; and it flexed 21% where the right hand flexed 20%. Leaving out the cases of 'no reaction,' the right hand flexed 34% and extended 65% while the left hand flexed 25% and extended 75%.

As for the head under pleasant stimuli, it was found that it flexed, or came forward, 29%, showed no reaction 26%, extended or drew back 45%. That is, in a total of 186 pleasant stimuli, the head came forward 54 times, drew back 84 times, showed no movement 48 times. Comparing the flexions with the extensions alone, the head under pleasant stimuli was flexed 39% of the times, and extended 61%. The head was more indifferent than the total averaged results, but more expressive than the right hand.

It will be noticed, however, that the head shows more of a tendency to flexion under pleasant stimuli than either hand, viz., 29% flexion to 20% for right hand and 21% for left hand. This was one of the surprises of the experiments. In many cases under pleasant stimulus the head would move forward even where both hands relaxed. At first it was thought this was true only of smells, as the odors were necessarily presented suddenly and in front of the face, but the same thing was true of colors and sounds; the head often flexed when these stimuli were agreeable. This must have been an adaptive movement; for often, after this forward movement, the head would drop back during the continuance of the pleasant stimulus.

2. *Under Unpleasant Stimulation.*

If we now examine the head movements in response to unpleasant stimuli, similar differences of reaction will be seen. Here it flexed 42%, showed no reaction 19%, extended 38%; or in a total of 168 stimulations it moved forward 72 times, showed no movement 32 times, dropped back 64 times. Taking flexion and extension alone, we find flexion 53% compared to extension 47%. Even more marked here is the tendency for the head to extend or draw back under an unpleasant stimulus than it was for it to come forward under a pleasant stimulus;

while both hands often, and indeed generally, flexed. Yet for the head also flexion predominates, thus supporting the theory by a slight percentage. Comparing the hands as to their reaction to unpleasant stimuli, we find the right hand here also, markedly more inhibited or indifferent. The left hand shows flexion $66\frac{2}{3}\%$, no reaction 14.5%, extension 18%; or in 165 cases it flexed 110 times, showed no reaction 24 times, extended 31 times. Leaving out the cases of 'no reaction,' it showed 78% flexion, and 22% extension.

The right hand, however, flexed only 49.5% of the times, while it showed 'no reaction' 29%, and extended 21%. Comparing flexion and extension cases alone the right hand flexed 69% and extended 30%. The right hand, therefore, shows more of a tendency to indifference and extension under disagreeable stimuli than does the left hand. We find then, in the hands a marked preponderance of flexions under disagreeable stimuli, 78% of the movements in the left hand and 69% of the movements in the right hand being flexions.

Combining the cases of the reactions of head and hands under unpleasant stimuli we get 53% flexion, 20% 'no reaction,' 26% extension; or, in 450 cases we have flexion 240 times, 'no reaction' 90 times, extension 120 times. Leaving out cases of no reaction we have $66\frac{2}{3}\%$ flexions to $33\frac{1}{3}\%$ of extension—a proportion of 2 flexions to 1 extension.

3. *Under Indifferent Stimulation.*

In the cases where the stimuli were pronounced indifferent, that is, neither agreeable nor disagreeable, we find some interesting results. Even here the left hand reacted more than the right. The left hand under indifferent stimuli shows flexion 34%, no reaction 31%, extension 34%; or in 32 cases flexion 11 times, no reaction 10, extension 11 times. It is interesting that flexion and extension are here exactly equal.

The right hand under indifferent stimuli was more immobile. It shows flexion 30%, no reaction 50%, extension 20%; or in the 20 cases of indifferent stimulation the right hand flexed 6 times, showed no reaction 10 times, extended 4 times. Leaving out the cases of no reaction it flexed 60%, extended 40%.

The left hand showed only 31% of 'no reaction' under indifferent stimuli while the right hand showed 50%. The head under indifferent stimuli showed 30% flexion, no reaction 30%, extension, 40%; or flexion 43% to extension 57%, if we disregard cases of 'no reaction.'

The total results of the hands and the head for indifferent stimuli are 31.7% flexion, no reaction 35%, extension 32.9%; or disregarding the 'no reaction' cases, flexion 49% to extension 51%. Extension and flexion here are almost equal with a slight percentage in favor of extension, probably due to adaptive efforts. This is what we should expect under indifferent stimuli. We find also here more cases where there was no reaction than occurred when the stimulus was pronounced pleasant or unpleasant, viz., 35% here to 26% under pleasant and to 20% under unpleasant stimulation.

4. Summary and Additional Observations.

The foregoing results may be summed up in the following table. It includes only the actual reactions to stimuli, 764 in number. The cases where stimuli were applied without resulting reaction numbered 253.

	UNDER UNPLEASANT STIMULI.	UNDER PLEASANT STIMULI.	UNDER INDIFFERENT STIMULI.
Flexion	66.6	32.2	49
Extension	33.3	67.8	51
Proportion	2 to 1	1 to 2 +	Nearly equal.

These experiments afford, therefore, a striking confirmation of Professor Münsterberg's theory, that there is a strong tendency to expansion under agreeable, to contraction under disagreeable, stimuli. Other tendencies are present, however, some of which conflict with this one: such for example as the tendency to move toward an object which attracts attention; the tendency to move away from a disagreeable object; the tendency to make particular movements of adaptation to stimuli; etc. A further influence of great interest is revealed upon examination of the records of the separate individuals who, as subjects, took part in these experiments. If their reactions to stimuli which

they pronounced indifferent be examined, it will be seen that some show a temperamental tendency to make movements of flexion more often than of extension; others, the opposite; and others still to make both in nearly equal proportion. These temperamental tendencies show themselves clearly in their influence on reactions to agreeable and disagreeable stimuli. The 'flexion' temperament shows, through the greater predominance of flexions, a greater difference in the proportion of the two movements under pleasant stimuli and a nearer approach to equality under unpleasant stimuli. The 'extension' temperament shows the opposite results, and the indifferent temperament exhibits proportions more nearly those given in the above table.

Temperamental differences then work together with the other special tendencies mentioned above in modifying the tendency to contract under disagreeable and to expand under agreeable stimuli. While, therefore, this latter is clearly established by this research as a real and strong tendency, it is at the same time shown to be only one tendency acting among many.

VISION WITHOUT INVERSION OF THE RETINAL IMAGE.¹

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Seventh Day.—In the morning the flow of ideas while I was blindfolded was like that described for the evening before. But I noticed in bathing that the old representation of those parts of my body which I had so frequently seen (at least in their clothing) during the experiment, was decidedly less vivid, the outline more blurred, the color paler, grayer, more 'washed out,' than of the parts which had never come within the limits of the visual field.

Later, with my lenses on, it seemed at first as if the experience was in all respects the same as on the previous day. But when I began to pace rapidly up and down the room, I felt that I was more at home in the scene than ever before. There was perfect reality in my visual surroundings, and I gave myself up to them without reserve and without being conscious of a single note of discord with what I saw. This feeling of complete harmony throughout, lasted as long as I kept my legs either within or near the borders of my field of view. Otherwise the older, inappropriate representation of my body arose at times, but faded, while the new representation revived, as soon as some passing object was seen to enter the region into which the older image of my body extended. The absence of any tactual experiences such as a real body in that position would imply, cast, for the moment, an illusory character over the older form of representation.

To what extent objects in view suggested the idea of other things in harmonious relation with the seen things is best shown by the following cases: As I walked into my bedroom and saw the bedstead, I involuntarily thought of the windows, repre-

¹ Concluded.

senting them in the appropriate direction fixed by the position of the bed. The general outlines of the room, and the more important points of reference, arose in harmony with the new sight-perceptions. But the detailed filling of this outline was far less complete than is usual in my case in normal sight. A large number of important things in the room simply did not arise in my mind until their relation to the field of seen things had been brought home afresh by perception. During the first days of the experiment ideas of objects frequently arose in opposition to the new sight-perception; now they either did not arise at all, or came in the newer form. The idea of the sofa or chair on which I *passively* sat did still come up in discord with the general experience, together with the dim feeling of my shoulders and of the upper parts of my back. But these were now a comparatively isolated group, and not a vigorous *Apperceptions-masse* to call up a host of surrounding things in orderly relation to itself.

In regard to movements, the most striking fact was that the *extent* of the movement now was inappropriate, movements in the wrong *direction* being comparatively rare in the case of the hands, and even still rarer in the case of the feet. My hands frequently moved too far or not far enough, especially when coming from beyond the visual field to something in sight. In trying to take a friend's hand, extended into the (new) lower portion of my visual field, I put my hand too high. In brushing a speck from my paper in the (new) upper portion of the field I did not move my hand far enough. And in striking with my index finger the outstretched fingers of my other hand the movement was much less accurate when I looked at my hands than when I closed my eyes and depended on motor guidance. The actual distance that my hand moved, in such cases, would, under the normal conditions of sight, doubtless have been appropriate to bring my hand to the desired spot. But an object in what had before been the upper part of the field was now at a shorter distance from my hands than formerly; the movement, under the influence of the habitual interpretation of the visual position, would therefore go too far. And, *vice versa*, a movement to an object in what had formerly

been the lower part of the visual field would now fall short of its destination. For the visual position would now require a more extended movement of the arm than formerly, in order to reach it.

When I watched one of my limbs in motion, no involuntary suggestion arose that it was in any other place or moved in any other direction than as sight actually reported it, except that in moving my arm a slightly discordant group of sensations came from my unseen shoulder. If, while looking at the member, I summoned an image of it in its old position, then I could feel the limb there too. But this latter was a relatively weak affair, and cost effort. When I looked away from it, however, I involuntarily felt it in its pre-experimental position, although at the same time conscious of a solicitation to feel it in its new position. This representation of the moving part in terms of the new vision waxed and waned in strength, so that it was sometimes more vivid than the old, and sometimes even completely overshadowed it.

The conflict between the old and the new localization of the parts of my body was shown in several instances. The mistaken visual localization of a contact in the palm of one of my hands, and the sudden reversal of even the touch-localization when I detected by sight the true source of the sensations, occurred as on the preceding day. Somewhat similarly, when I moved a heated iron with my right hand to that border of the visual field just beyond which, according to pre-experimental localization, my left hand would have been lying, I involuntarily felt an anticipatory shrinking in my unseen left hand, as if it were on the point of being burnt; although the iron in my right hand was actually several feet from my left, and was moving away from it. When I put my left hand in sight, or looked at it afresh to make sure where it was, the hot iron caused no premonitory feeling whatever on approaching the visual locality which had before been so suggestive of danger.

Seated by the open fire, I happened to rest my head on my hands in such a way that the fire shone directly on the top of my head. I closed my eyes, and the image of the fire remained true to the recent perception. But soon I noticed that I was

representing the fire in pre-experimental terms, and I finally discovered that the change was caused by the growing sensations of warmth on the top of my head. My hair and scalp were persistently felt in their older position, no doubt because I never directly saw them in any other. And the old localization of the fire was the only one consistent with this old localization of the hair and scalp. But by passing my hands rapidly back and forth before my open eyes, ending the movement each time with a touch upon the top of my head, it was not difficult to produce a vivid localization of my scalp in harmony with the new sight-perceptions. And with this change the old localization of the fire was suppressed. During the walk in the evening, I enjoyed the beauty of the evening scene, for the first time since the experiment began. Evidently the strangeness and inconvenience of the new relations no longer kept me at such a tension as hitherto.

On removing the glasses, my visual images relapsed into their older form, with a constant interplay and accompaniment, however, of the new.

Eighth day.—Before putting the glasses on, representations of the older sort held sway.

During the morning, after the glasses were in place, I noticed that as far as the unseen portions of my body were concerned, the relation of right and left was, for the most part, a reproduction of the older visual right and left; that is to say, a contact on the right side of the body at some point beyond the reach of sight was felt and visually represented on the (old) visual right side. Occasionally the opposite visual side was suggested, but the sensations were rarely indeed felt there. The case was quite different as regards the seen parts of my body, although even here uncertainly and sudden alteration of reference occurred. The illusion of contact on the opposite hand to the one actually touched, arose as on the two preceding days. I often hesitated which hand was the appropriate one for grasping some object in view, began the movement with the wrong hand and then corrected the mistake. If I was attentive to the new visual representation of some part of my body which was about to be touched, and expected the contact

there, the contact was felt in the new position and no change of reference occurred. Immediately afterwards there usually arose a sort of tactual after-image on the other visual side. When the original contact was unexpected, the visual image and the tactual localization might simultaneously be both old and new, or might be old alone, with perhaps a merely visual image in the new direction, although without any real reference of the touch-sensations to this image.

Localization of sounds varied, being different when the source of sound was in sight from what it was when this was out of sight, and also in the latter case differing with different directions of attention, or with different suggestions as to the direction from which the sound came. The fire, for instance, sputtered where I saw it. The tapping of my pencil on the arm of my chair seemed without question to issue from the visible pencil. Even when I tapped on the wall to one side, out of sight, if in making the stroke I invariably passed my hand and pencil before my eyes and in the direction of the unseen part of the wall, and attempted to picture the contact in harmony with this movement, I actually heard the sound come from the new visual direction, although not with full and unequivocal localization. There was a strong temptation to localize the sound on the other side also. And this rival localization rose into full life the instant I ceased to keep before me the image of the pencil striking on the new visual side.

The influence of the suggestion coming from recent and repeated movements before the eyes was likewise apparent in localizing parts of my body which could not be brought into the visual field. Thus the involuntary inattentive localization of my forehead and hair was the old localization lasting from pre-experimental sight. But a series of visible movements of my hand to my hair, together with fixed attention on the goal of these movements, made the sensations of touch temporarily come, without difficulty, from this new direction. Sensations of contact on the lips, however, were not so readily dislodged from their old position. In eating at table, the movements of my hands and of pieces of food across the visual field, constantly suggested that my mouth must lie between the line of

sight and the new position of my legs. But the actual contact on my lips instantly dispelled this suggestion and located my mouth definitely and indubitably on the other side of the line of sight. The place of the actual contact and that of the merely suggested contact were thus in striking contrast. But when I did my best to visualize my lips in the direction of the suggested contact and strained my attention in this direction, the actual contact did not dissipate this image or carry it to the old position of my mouth, but the touch-sensations seemed to come from the new direction. Without such a willful visualization and strain of attention the actual contact always reversed the involuntary suggestion coming from the visible movements toward the new position of my mouth. Even when my forehead and hair temporarily seemed to lie on the (new) upper side of the line of sight, this did not prevent my mouth from being felt on the *same* side. But the new localization of forehead and scalp undoubtedly had a tendency to drive the mouth out of its old localization; for I found that less effort of attention and visualization was required to make the tactual sensations of the lips come from the new position, when the top of the head had already been carried over to its new position. No doubt there was a disturbing incongruity in having both my mouth and the top of my head on the same side of the line of sight; consequently the re-localization of one tended to carry the other to the opposite side of the visual line.

In other cases the re-localization of bodily parts that were beyond the reach of sight was brought about by the suggestive influence of such movements as I have just described, without any voluntary attention or visualization of the parts whatever. As I rocked in my chair, I found that by throwing my arms up through the field of sight into the visual region in which my shoulders, according to the old experience, were wont to be localized, the repeated feeling of the unimpeded motion of my hands through this region destroyed the old representation of my shoulders and back, and gave them a localization in harmony with the new visual experience, except that (as I noticed) my head seemed too deep-set in my shoulders—in fact, seemed buried in them almost up to my ears.

The harmonization of the new experience and the suppression or subordination of insistent remnants of the old were always apparent during active operations in the visual surroundings, as has been described for several of the preceding days. While I sat passively the old localization of unseen parts of my body often came back, or perhaps was the usual form in which they appeared. But the instant I began to rock my chair the new position of these parts came prominently forward, and, except in the case of my shoulders and back, readily felt more real than the old. And in walking, when hands and feet rhythmically made their appearance in the visual field, the old representation, except perhaps for some faint inharmonious sensations in the back, was fully expelled without employing any device of will or of attention whatever. The attempt to represent my body in its older form or position ended in a faint, lifeless outline, deficient, as far as I could make out, in those parts which (in a different direction, of course) were actually in sight. The sight of these parts made it impossible to represent them in harmony with the older experience. If in walking I allowed my feet to remain outside the field of view and they relapsed into their older localization, they returned, although still unseen, to their new position as soon as I approached a step or other slight obstacle on the floor.

As long as the new localization of my body was vivid, the general experience was harmonious, and everything was right side up. But when, for any of the reasons already given—an involuntary lapse into the older memory-materials, or a willful recall of these older forms—the pre-experimental localization of my body was prominently in mind, then as I looked out on the scene before me the scene was involuntarily taken as the standard of right directions, and my body was felt to be in an inharmonious position with reference to the rest. I seemed to be viewing the scene from an inverted body.

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When the time came for removing the glasses at the close of the experiment, I thought it best to preserve as nearly as possible the size of visual field to which I had now grown accustomed; so that any results observed might be clearly due solely to the

reversion of my visual objects and not to a sudden widening of the visual field. Instead, therefore, of removing the plaster-cast from my face, I closed my eyes and had an assistant slip out the brass tube which held the lenses, and insert in its place an empty black-lined paper tube that gave about the same range of vision. On opening my eyes, the scene had a strange familiarity. The visual arrangement was immediately recognized as the old one of pre-experimental days; yet the reversal of everything from the order to which I had grown accustomed during the past week, gave the scene a surprising, bewildering air which lasted for several hours. It was hardly the feeling, though, that things were upside down.

When I turned my body or my head, objects seemed to sweep before me as if they themselves were suddenly in motion. The 'swinging of the scene,' observed so continuously during the first days of the experiment, had thus returned with great vividness. It rapidly lost this force, however, so that at the end of an hour the motion was decidedly less marked. But it was noticeable the rest of the day, and in a slight degree even the next morning.

Movements which would have been appropriate to the visual arrangement during the experiment, were now repeatedly performed after this arrangement had been reversed. In walking toward some obstacle on the floor of the room—a chair, for instance—I turned the wrong way in trying to avoid it; so that I frequently either ran into things in the very effort to go around them, or else hesitated, for the moment, bewildered what I should do. I found myself more than once at a loss which hand I ought to use to grasp the door-handle at my side. And of two doors, side by side, leading to different rooms, I was on the point of opening the wrong one, when a difference in the metal work of the locks made me aware of my mistake. On approaching the stairs, I stepped up when I was nearly a foot too far away. And in writing my notes at this time, I continually made the wrong movement of my head in attempting to keep the centre of my visual field somewhere near the point where I was writing. I moved my head upward when it should have gone downward; I moved it to the left when it should have

gone to the right. And this to such a degree as to be a serious disturbance. While walking, there were distinct signs of vertigo and also the depression in the upper abdominal region, noticed during the earlier days of the experiment. The feeling that the floor and other visual objects were swaying, in addition to the symptoms just mentioned, made my walking seem giddy and uncontrollable. No distinct errors in localizing parts of my body occurred; I was more than once surprised, however, to see my hands enter the visual field from the old lower side.

Objects in the room, at a distance of ten or twelve feet from me, seemed to have lost their old levels and to be much higher than they were either during the experiment or before the experiment. The floor no longer seemed level, but appeared to slope up and away from me, at an angle of perhaps five degrees. The windows and other prominent objects seemed also too high. This strange aspect of things lasted (as did also the swinging of the scene, the feeling of giddiness, and certain inappropriate movements) after the plaster cast had been removed and the normal compass of the visual field was restored. In the dim light of the next morning, the upward slope of the floor and the unusual position of the windows were distinctly noticeable.

It is clear, from the foregoing narrative, that our total system of visual objects is a comparatively stable structure, not to be set aside or transformed by some few experiences which do not accord with its general plan of arrangement. It might perhaps have been supposed beforehand that if one's visual perceptions were changed, as in the present experiment, the visual ideas of things would without resistance conform to the new visual experiences. The results show, however, that the harmony comes only after a tedious course of adjustment to the new conditions, and that the visual system has to be built anew, growing from an isolated group of perceptions. The older visual representations for the most part have to be suppressed rather than reformed.

Why then do the old visual ideas persist in their old form, and not come immediately into accord with the new perceptions?

If their position were merely relative to the sight-perceptions, they undoubtedly would come into harmony with these perceptions, at least after the first moments of dismay were past. But the fact that the ideas can for some time refuse spatially to conform to the new experience, shows that their position and direction is fixed with reference to something other than the immediate perceptions of sight. What is it which caused the older visual images to preserve a spatial arrangement whose lines of direction were opposed to those of the actual field of view?

To say that the older visual directions persisted because the older tactual directions remained in force, is certainly no sufficient answer unless we can show that visual direction is dependent on tactual direction. But the preceding narrative furnishes strong evidence against such a view. If there is any dependence either way (which I doubt), the evidence seems to favor the primacy of sight.

However that may be, the facts in the present case are more accurately described when we say that the discord was not between tactual directions and visual directions, but between the visual directions suggested by touch and the visual directions given in the actual sight. The real question then is: Why did touch-perceptions so persistently suggest visual images whose positions and directions were in discord with the actual scene? The answer is found, I think, in the familiar doctrine of 'local signs' in touch and in sight, and in the farther assumption that a system of correspondence exists whereby a sign in one sense comes to be connected with and to suggest a particular sign in the other sense.

In the organized experience, a perception in one sensory field not only has in it that peculiar qualitative or intensive character which is its own 'local sign,' but, through this local sign, suggests in the other sensory field the local sign which is most intimately associated with the first. A perception in one sensory field suggests, therefore, in terms of the other sense an image in that place whose local sign is most strongly associated with the local sign of the original perception. According to this view, the local signs of sight correspond to the signs of touch, and *vice versa*; so that each member in this system of *corresponding*

signs has its particular correlate in the other sensory field. The correspondence here indicated, does not, however, consist in any spatial or qualitative identity or even similarity of the particular signs which correspond, but only in the fact that both have come to mean the same thing. They have occurred in connection with disparate sensory perceptions whose times of appearing and whose 'curve' of change have been so continuously and repeatedly identical that the perceptions themselves come, in time, to be referred to the same source, or, in other words, give the perception to the same object. The perceptions of the two senses are thus identified; and, at the same time, the disparate local signs (in the different senses) which are simultaneously aroused in the perception of the one object come to have the same spatial meaning. This correspondence of local signs is no doubt an important condition for our perceiving one and the same thing in different sensory fields. And the persistence of this correspondence between the signs, when once the power of mutual suggestion has become established, is the explanation of the fact that during my experiment the translations of touch-perceptions into terms of sight continued so long in contradiction to the actual visual experience; and that, on the other hand, the visual perceptions so long suggested tactual or motor images not in accord with the tactual or motor perceptions.

For, whatever the local signs of vision may be—whether differences in the qualitative or intensive character of the muscular sensations, or differences of sensation connected with different parts of the retina, or intricate combinations of both of these materials—the reversion of the retinal image would so alter the conditions of sight that the tactual perception of an object and the simultaneous sight of the same object would no longer call into play the pair of local signs which hitherto had had the same spatial meaning, but a pair of signs which had come to have opposed spatial meanings. Suppose, for illustration, that any two tactual local signs,¹ *a* and *b*, have in my nor-

¹For convenience sake let us speak of the signs as though they could be simple. Of course they are really complexes of sensations from joints and muscles and skin. Similarly of the visual signs.

mal experience the same meaning as the two visual local signs m and n respectively. A single object which is both seen and touched will arouse the synonymous signs a and m , or subsequently the pair b and n , supposing that the object changes its place. If I merely touch the object, in such a place that a is aroused, this suggests by associative correspondence the visual sign m ; and when subsequently I look at the object, m is actually aroused, and the place of the visual object is seen to be identical with the visual place suggested by touch. Touch in this case suggests a visual experience which the visual perception confirms. Likewise a visual perception whose local sign was n would suggest, and afterwards be followed by, a tactual experience whose local sign was b . And in these cases the spatial character of the perceptions by the different senses would be harmonious.

But suppose, now, that the retinal image is changed, as in the experiment. An object which arouses the tactual sign a will no longer give a visual experience containing the sign m , but will give one containing, say, n . And the visual experience containing the local sign m is no longer accompanied by a tactual experience containing a , but by one containing, say, b ; and *vice versa*. By the long previous experience, however, a touch-perception containing the sign a has come to suggest a visual experience containing the sign m , and will consequently continue for some time to suggest such a visual experience. But the actual sight of the object will show it in a different place from what touch suggested; for the visual experience will now actually contain n and not m . And likewise this visual experience whose local sign is n will for some time continue to mean a tactual experience whose sign is b , in a different locality from the real touch of the object, which now contains the sign a . Each sense will in this way suggest experiences which the actual perceptions of the other sense will contradict. Thus touch and sight will be in mutual discord.

According to the view here presented, this discord will continue as long as the local sign a suggests the local sign m , and *vice versa*. But when a , by repeated connection, suggests only the visual position implied in the local sign n , and this latter in

turn means only the touch locality whose sign is a ; and when m and b have come to have an identical meaning, or are in correspondence; then the total experience will again be harmonious. Each sense would then suggest only what the other sense would confirm. We would see things where we felt them to be, and we would feel them where we saw them to be. But until this reharmonization has been brought about, visual ideas in the older form will continue to arise at the suggestion of tactual experiences, and there will be discord between the things in sight and the wider system of visual representations. The persistence of the old inter-sensory correspondences accounts, therefore, for the long opposition of visual ideas and visual perceptions during the experiment.

We are now enabled also to see what the harmony between touch and sight really is. The experiment clearly shows that an object need not appear in any particular position in the visual field in order to admit of a union or identification of the tactual and visual perceptions of the object. The visual position which any tactual experience suggests—the visual place in which we ‘feel’ that an object is—is determined, ~~not by some fundamental and immutable relation of tactual and visual ‘spaces,’~~ but by the mere fact that we have constantly seen the object there when we have had that particular touch-experience. If this particular touch-experience were the uniform and exclusive accompaniment of a visual object in some different visual position, the two sensory reports would mean the same thing, and the places of their object would be identical. Of course, the harmony of touch and sight also implies that visual appearances have the same relations to one another as tactual appearances have to one another; so that a given object in sight must have the same spatial relation to the rest of my visual world as the accompanying touch-object has with respect to the rest of my tactual world. But this harmony does not require that the visual manifestation of a tactual object should be just here and not there, or in this direction and not in that.

The inverted position of the retinal image is, therefore, not essential to ‘upright vision,’ for it is not essential to a harmony between touch and sight, which, in the final analysis, is

the real meaning of upright vision. For some visual objects may be inverted with respect to other visual objects, but the *whole system* of visual objects can never by itself be either inverted or upright. It could be inverted or upright only with respect to certain non-visual experiences with which I might compare my visual system—in other words, with respect to my tactual or motor perceptions.

The reharmonizing of touch and sight, in the experiment, consisted therefore of a double work. Visual objects and ideas, which were at first isolated, had to become a system whose parts had the same relations among themselves as the parts of the tactual system, or of the older visual system. Not until the construction of a visual system enveloping and supplementing the actual visual field, would sight have something corresponding to the touch-system brought over undisturbed from the older experience. But the completion of this work was dependent on the progress of the second work, namely the perfecting or entire reconstruction of the process of translating from sight into touch and from touch into sight. Until this reconstruction was complete, each sense would suggest experiences of the other sense which this other sense would flatly contradict. Their reports would therefore necessarily seem discordant. But the restoration of harmony between the perceptions of sight and those of touch was in no wise a process of changing the absolute position of tactual objects so as to make it identical with the place of the visual objects; no more than it was an alteration of the visual position into accord with the tactual. Nor was it a process of changing the relative position of tactual objects with respect to visual objects; but it was a process of making a new visual position seem the only natural place for the visual counterpart of a given tactual experience to appear in; and similarly in regard to new tactual positions for the tactual accompaniment of given visual experiences. New associations had to develop, and new forms of expectation had to arise; in a word, new correspondences had to be brought about. But the tactual perceptions, as such, never changed their place. They simply got a new visual translation.

The especial obstinacy of the old representation of the body

requires no extended comment. It is what we would expect when the cause of the persistence of the older images in general is understood. If visual suggestion from touch, based on the pre-experimental set of correspondences between touch and sight, was the chief cause of the persistent return of the older images, then of course the conditions were peculiarly favorable for a continuance of the old visual representation of the body. For in this case touch was ever-present; and moreover the body was seen only in part. Head, neck, shoulders, and the upper part of the trunk, could not be directly seen at all. Shadows, reflections, etc., had some influence in bringing to mind the new visual place of these parts; but such indirect agents lacked the force of direct and continued perception. So that the possibility of bringing about new correspondences was confined, for the most part, to my arms and legs. But there is, doubtless, a solidarity of the body, and when so large a part could not be reached by the new experience, the rest also was affected but little. The body hung together as a unit, and refused to go with the new, unless all of it could go.

In the daily experience during the experiment, localization of parts of the body to one side (right or left) of the visual field of representation was more persistently in discord with the new visual experience, than was the vertical localization of these parts. The explanation of this is found, I think, in the fact that both tactual and visual differences in the body are much more striking at different levels of the body (passing up and down) than on different sides of the body. I frequently saw one of my hands and took it for the other, but of course I never looked at my foot and thought it was my hand. So, too, I erroneously localized in one of my hands an object which was in contact with the other hand, but I never localized in my foot a contact that was really on my hand, nor *vice versa*. (The experience itself was thus, as far as lateral relations were concerned, comparatively ambiguous, but not ambiguous at all with respect to vertical relations. When, through the touch-experience, a visual image was called up in the old lateral relations, this image was not so violently in discord with the new visual experience, as a mistake in vertical reference would have

been. For the image of my right arm or leg would also serve fairly well for that of my left. And in actual sight nearly the same objects were seen now on one side and now on the other. This hindered a fixed association of a particular image with a particular visual side, such that when a contact occurred the part of the body which it suggested must be referred to this side and not to that, if the contact were to fit into the visual total at all. The fact that the new visual experience was tolerant of a localization on either side almost indifferently was therefore favorable to a continuance of the old lateral localization. But the new vision unequivocally pronounced against an error in vertical localization; the uniform contradiction tended therefore to break up the old suggestions, and to build anew the vertical system more rapidly than the lateral.

It is not improbable that the persistence of the old lateral localization of the body was the main cause of the relative permanence of the old localization of sounds. For, vertically, wide changes in localization of sounds readily came by spontaneous suggestion; but not so the lateral changes. These came rarely except by strong effort of attention and voluntary visualization. The chief basis for determining the side from which a sound comes—the relative intensity of the sound in the two ears—would lead us to expect exactly this result as long as the two sides of the body were involuntarily represented as of old. If a sound was localized with reference to a particular ear or side of my head, then it would be localized in the old way as long as these were localized in the old way; and not until the localization of the two ears or sides of the head was transposed into harmony with the new experience would the auditory localization, at least in its lateral aspects, come into harmony with that experience. The cases in which the lateral localization of sound did accord with the new sight were no doubt due to a momentary strengthening of the influence of visual suggestion to such a degree that the usual dominant factor in lateral localization became subordinate. The fact that the sound of my footsteps conformed to the tactual and visual localization of my feet, and that in general the sight of the sound's place of origin carried with it the localization of the sound, shows to what an extent our auditory localization is influenced by suggestion.

It was repeatedly noticed in the course of the experiment that the total experience was much more harmonious during active movements of my body than when I inactively looked out upon the scene. This becomes intelligible when one sees how such movements gave additional vivacity to the new visual experience and to all that was in harmony with it, and tended to suppress those images of the body which did not accord with the new relations. The movements of my arms and legs into and through the visual field emphasized their new visual position more than their motionless appearance would have done. They caught and held the attention, and by the vigor of their appearance suggested the rest of the body in harmony with themselves. Moreover the movements of the field of view, when I nodded my head, or moved up and down in walking, or rocked in my chair, were such as harmonized with the movement of my body only when my body was thought of as in its *new* visual position. Otherwise the objects passed through the field of view in the wrong direction,—in a direction which the felt movement of my head or body did not at all explain. And, finally, the new localization of the body was the only one which was *practically important* when the visible environment had to be actively encountered. My actions could be guided, not by keeping in mind the pre-experimental localization of the body and noticing its relation to objects in sight, but only by accepting the new position of my body as *real* and constantly watching its relation to surrounding things. The scene itself became more my own by acting upon it, and this action reacted to bring the representation of my body into harmonious relation to the scene. As a result of these various influences, the whole experience was cleared of inner discord to a degree seldom if ever attained during a time of repose.

At the close of the experiment, after the lenses had been removed, windows and other prominent objects, as the narrative recounts, seemed too high. This was puzzling enough until I discovered that, when my apparatus was on my face, objects in the centre of the field of view were slightly lower than when seen without the lenses. The axis of the cylinder containing the lenses was in fact not exactly the same as the line of sight

when the eye was directed to the centre of the field. The displacement this caused in the apparent position of things was the less noticeable because of the general transposition of objects by the inverting power of the lenses. But a careful examination showed that the downward displacement at the centre was about equal to the apparent upward displacement on removing the lenses. No exact comparison could be made, for I did not notice this peculiarity of the apparatus until several days after the close of the experiment. So that I had to depend on my inexact recollection of what the extent of the illusion had been. It is, of course, barely possible that the illusion was in some way a direct consequence of reinverting the retinal image. But more probably the general displacement of objects, by reason of the position of the tube, had grown so familiar that their normal position on removing the glasses seemed as much too high as their position during the experiment had been too low.

But to return to the more significant features of the experiment. These are, without doubt, found in the results bearing on the relation between touch and sight, and through them on the interrelation of the senses generally. The experiment makes it clear that the harmony between sight and touch does not depend on the inversion of the retinal image. The spatial identity of tactual and visual objects evidently does not require that there should be a visual transposition of objects or that they should be given some special direction in the visual field. The chief reason for the existence of the projection theory is therefore taken away. Nor, on the other hand, are the visual directions made known to us and determined through our perceiving the 'absolute,' or pure motor, direction of the movements which alter the line of sight. The facts all go to show that the direction of movements of the head or eyes is not judged on purely muscular evidence, independently of the simultaneous changes in vision itself. On the contrary the movements are soon felt as having a direction opposite to that of the objects passing through the visual field. During the experiment, for instance, I often felt my eyes turn toward the sky and away from my feet, although they really turned toward my feet. The felt direction of the movement is therefore relative to the

direction of the movement of visual objects, and the 'absolute' muscular direction cuts no decisive figure in the perception at all. This will no doubt seem a hard saying to those who have been pinning their faith more and more on the unimpeachable witness of muscular sensations. It certainly makes the eye-movement doctrine of visual directions of little practical assistance for understanding the harmony between sight and touch.

This harmony, as was said, seems rather to be an accord of the *ideas* suggested in terms of one of the senses, with the *perceptions* of the same sense. When touch and sight agree, it means that the perceptions of sight are spatially identical with the visual suggestions produced by touch, and that the perceptions of touch spatially identical with the tactual suggestions produced by sight. The doctrine of a correspondence of local signs, stated some pages back, makes it easy to see how such a harmony could grow up; and, at the same time, how a reharmonization of touch and sight is possible, whatever may be the position of the retinal image. The view makes provision, therefore, for the special results of the experiment, as well as for the normal course of our experience; which the current doctrines concerning the interplay of touch and sight seem hardly able to do.

THE PSYCHOLOGY OF SOCIAL ORGANIZATION.

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The object of this paper is to present in outline a way of conceiving of the general fact of human social organization in line with the tendency which has proved itself fruitful in the last few years mainly in France; the tendency to recognize the psychological character of the *motifs* at work in society. It seems to me to be a permanent advance that the biological analogy is giving place to a psychological analogy, and that this is leading the writers in so-called 'sociology' to examine the psychological processes which lie wrapped up in the activities and responsibilities called social. The point of view sketched in the following pages reproduces some parts of a work entitled 'Social and Ethical Interpretations in Mental Development,' which is now in press.¹

§ 1. The hard questions, to the thinker about society, seem to me to be two, each of which should have a two-fold statement. The first question concerns the *matter* or *content*, of social organization; what is it that is organized? what is it that is passed about, duplicated, made use of, in society? When we speak of a social phenomenon in its lowest terms, what is it all about, what is the sort of material which must be there if society is there? This question has had very acute discussion lately under the somewhat different statement: what is the criterion or test of a social phenomenon? But the question which I ask under this head is more narrow, since, in all sorts of organization, a further question comes up in addition to that of the matter—the further question, *i. e.*, as to the processes, methods of

¹ Macmillans. Seeing that this paper was prepared, in the first place, for a sociological Journal (the *Rivis. Ital. di Sociologia*) the more purely psychological parts of the work are not given much notice. The psychological chapters consist, however, partly of further developments of points of view contained in my earlier work *Mental Development in the Child and the Race*.

functioning and laws of organization of the social content or material. It has been the weakness of many good discussions of late, I think, just as they did not set these questions separately, *i. e.*, (1), the matter, and (2), the functional method.

Let us take an illustration. Some of the animals show a certain organization which appears to be social. But on examination, in certain instances, we find that the actions involved are purely hereditary, congenital, each animal doing his part, in the main or altogether, simply because he is born to do it whenever the organism becomes ripe for these actions under the stimulation of his environment. Now let us contrast with this the intelligent coöperative performance of the same actions by a group of men or children who deliberately join to do them in common. In these cases it is clear that the matter of organization is different; one being a purely biological and instinctive, the other a psychological and acquired action. The results to the observer may be the same, and the question may still remain as to whether the functional method be the same or no, but there is no doubt that the matter is different in type. These two questions may therefore be distinguished at the outset with so much justification.

§ 2. But each of these two questions allows of a two-fold requirement. If we assume that the distinction between habit (with relative fixity of function) and accommodation (with relative plasticity of function as seen in all progress in learning or acquisition) holds of society, then both the matter and the method or process of social organization must allow of these two modes, and working together must besides produce them. If, for example, we take an individual and find that he has a habit of acting in a certain way, and at the same time improves upon his action from day to day, we yet say that the action remains the same in its content or meaning throughout the entire series, from the fixed habit to the skilled variation. Our determination of the content of the action must have reference to just the possibility of the entire series of statements, from fixed repetitions of habit to the extreme variations of accommodation, through all the intermediate stages. In other words, the fact of growth by a series of accommodations must be reck-

oned with in all the determinations of social content. And statements of *progress* must go with the definitions of the actual content at any given stage of social organization. In other terms, the content of social life is a changing growing content, and the definition of the material of social organization must take account of this character.

And so must the theory of the methods of functioning also. The process of social organization results in a growing developing system. Progress is real, no matter what its direction, provided it result from the constant action of a uniform process of change in a uniform sort of material. This we find in social life, and this is the prime requirement of social theory both in dealing with matter and in dealing with function.

§ 3. It may suffice to bring these distinctions and the problems which emerge more clearly to the light, if we note briefly some of the later attempts to deal with the social organization from a psychological point of view. I shall cite types of theory, referring to particular writers merely as illustrating these types and without going into the details of their positions.

The Imitation Theory, illustrated by M. Tarde. This view of social organization has very much to commend it, from the point of view of method; indeed, as will appear in the sequel, I think it is with modifications of statement the true, and possibly the final solution of the question of type of social function. As a complete doctrine of society, however, it fails signally, since it gives no answer to the question of matter. M. Tarde does not tell us what is imitable, what is capable, through imitation, of becoming fixed as social habit, and also of being progressively modified in the forms of social progress. He does seem to become more aware of the need of answering this question in his later work, *La Logique Sociale*, and introduces certain elements of content as 'beliefs and desires,' to supply the lack. This, however, means simply a departure from his earlier theory, in which the phenomenon of imitation was treated as an answer to the question *qu'est ce qu'une société?* Apart from M. Tarde's personal views, it may be said that the case of imitation at its purest is just the case in which the social vanishes. Imagine a room-full of parrots imitating each other in regular

sequence around the area, and let them keep it up *ad infinitum*, and with as much individual variation as they may; where is the social bonds between the parrots? In so far as the imitation is exact, in this case a thing of congenital instinct, in so far we might substitute tuning-forks for the parrots, and let them vibrate together after striking one of them a sharp blow. Indeed, in his treatment of the final nature of imitation in his *Lois de l'Imitation* M. Tarde brings it into a sort of cosmic correlation with undulatory repetition in physics. I can not see that the mere presence of imitation would avail anything, without tacit or explicit assumptions of two kinds: first, that the material of social organization is essentially imitable material; and second, that through imitation this material would take on the forms of organization actually found in society.

2. Another type of theory which is open to the same criticisms in effect is represented by the 'constraint' view of M. Durkheim. To this view the essence of social organization is the constraining influence of one person upon others. It is in line with the extreme 'suggestion' theory of society, which makes the crowd acting under the suggestion of the strongest personalities in it the type of social organization as such. The weakness of this type of doctrine appears from the striking analogy from hypnotic suggestion which its advocates employ. And the element common to such a view with that of M. Tarde is evidenced in the use which he makes of the same analogy. The analogy seems to me to be quite correct; to this view the extreme and the purest instance of social organization would be hypnotic *rapport*. Here constraint is well nigh absolute, imitation is perfect, subordination is unquestionable. But it is only necessary to state this to see that in hypnotic rapport the social has completely evaporated. It gives no criticism or criterion of social material; the hypnotic subject or the generally suggestible subject tends to take all suggestions as of approximately equal value, to obey everything, to understand nothing, to be the same sort of an instrument of repetition as the parrot and the tuning fork. How there could be any organization as distinct from repetition, of progress as distinct from arbitrary caprice, I am quite unable to see. It may be, as a matter of history, that the first social man

became so because he was knocked down by a stronger, and so constrained to be his slave; but further progress from such a state of constraint, in the direction of coöperation, would be possible only in proportion as there was a 'let-up' or modification of the one-sided constraint. In other words constraint—or rather the imitation to which it may be reduced as soon as it ceases to be one-sided and becomes *mutual*—may have been the method and may continue to be the method of social organization, but the lines of progress actually made by society would seem to be determined by certain inherent possibilities of fruitful coöperation and organization in some particular spheres. These spheres must be defined, and that raises the quite different question of matter or content. The constraint theorists, I know, take as type of constraint not that of force but that of suggestion; and it is just this tendency which brings their view into line with the imitation theory and makes it available as an important, but less important, contribution to that theory.

3. There is another way again of looking at social organization, a way which, however, may be called psychological only with some latitude. Dr. Simmel, of Berlin, may be taken as representing it in a part of his treatment of society. It consists in attempting, by an analysis of social events and phenomena, to arrive at a statement of the formal principles which each section or general instance of social life presents. Such formal principles are division of labor, altruistic endeavor and coöperation, etc. This is a very serviceable undertaking, I think, and must result in a certain valid social logic—a system of principles by which social phenomena may be classified and which may serve as touch-stones of particular cases of organization. The objection, however, to building a science of sociology upon it is just that the principles are formal; it would be like building the psychology of concrete daily life upon the formal principles of logic. Principles which get application everywhere are not of concrete use anywhere. They also lack—or the system which seeks them out lacks—the genetic point of view. Granted the establishing of these principles by the analysis of social events, the question would still remain as to the original form which they showed in primitive societies. It is

easier to deal with the simpler and work up than it is to reverse this procedure; and from this point of view it would seem quite possible to treat all such principles as developments from imitation and suggestion. Apart from this, however, the essential criticism to be made upon this type of thought is that it deals only with form and functional method and assumes certain sorts of matter of social organization. The principle of division of labor, for example, assumes conscious *thought*, in such division and its constant application by the members of society.

4. Another class of doctrines have the merit of being genetic, those which found the social life of communities upon certain primitive emotions, such as sympathy. These theories are exemplified by Mr. Spencer, M. Novikow and the English moral philosophers. This is possibly the oldest form of social theory, having its roots in Aristotle, and has all the accumulated authority of age. Its forms of statement are also so numerous that I cannot take them up. From the pure 'sympathy' theory we pass to the 'altruistic theory' which makes social life a derivative of ethical; to the social instinct theory, which says that man is natively social, and sympathy and altruistic feeling are evidences of it; and finally we reach the climax of descriptive vagueness—in a formula wide enough to include all the rest—the 'consciousness of kind' recently propounded by Professor F. Giddings.¹

As a class it may be said of all these theories that they constantly confuse the questions of method and matter in social organization. In regard to method of function the imitation theory comes in at once to supplement these earlier points of view. But apart from this lack it may be said that the life of feeling and instinct does not furnish the requirements of matter for social organization. There are two sorts of sympathy, two sorts of social instinct, two sorts of consciousness of kind. This appears when we press the requirement indicated above that the matter of social organization should be such as to allow the formation both of social habit and of the adaptations seen in social accommodation and growth. The life of instinct as such

¹ In the third edition of his *Princ. of Sociology* (Preface), however, Professor Giddings defines 'consciousness of kind' more in terms of sympathy.

and of the emotions which come with instinctive activities—*e. g.*, organic sympathy, impulsive altruism, manifestations of kind, such as maternal affection, etc.—all these are race habits. To the degree in which they fulfill the requirement that society live by its stock of habits, to that degree do they fail to enable society to modify its habits and grow. If we sympathize with each other by pure instinct, and act on the movings of sympathy, new organization would be as far off as if we fought tooth and nail; for action would be as capricious. So also, merely to feel socially inclined would not beget differential forms of social organization. To be conscious of others as of the same kind would in itself not determine, in the slightest degree, the sort of thought or action which should be fruitfully recognized and developed within the habits of the kind. If we assume an adequate content, a common material, in short, if we assume social organization already, in the groups which for convenience *after they are made up in nature*, we call kinds, then of course it is the simplest thing in the world to say that what the members have in common is their consciousness of kind; but is no more an explanation than is the phrase 'love of drink' an explanation of inherited tendency to alcoholism.

It is only when we come to see the second or higher sort of sympathy, social instinct, consciousness of kind, etc., that the requirement that social organization be progressive becomes more apparent, because only there is it possible of fulfillment. We do not find instincts getting much organization apart from certain fixed and congenital forms of association. The higher emotions and actions which arise when consciousness becomes in some degrees reflective as opposed to instinctive, take on aspects which are differentiated from one another according to the mental content which they accompany. There is a reflective sympathy, a reflective sociality, a reflective consciousness of kind; and it is just their value that they now afford some criterion—a material criterion—over and above the mere fact of feeling and instinct. This point is the main business of this paper, so I need not dwell upon it here; but it leads us to see that the theories which deal in such general descriptions of social organization as the terms mentioned carry, are quite in-

adequate, since they leave the real question of matter unanswered: of the 'what' of social organization—the 'what' of such questions as "what does society fruitfully imitate?" "what feeling and acts of sympathy yield results of social value and permanence?" "what is the something found sometimes in the consciousness of kind which in these cases leads to the sort of progress characteristic of an ethical society as opposed, let us say, to a school of fish?" Of course I am not intending to draw lines, even between the ethical society and the school of fish. It is a further question, after we determine the what of social organization, to find how far it is present also in the behavior of the school of fish. But what is it?—'that is the question.'

§ 4. This brief characterization of theories, all aiming to be psychological, enables us to see our problem. I have introduced them only for this purpose; and the inadequacies of presentation will, I hope, not be construed as inadequacies of appreciation. The way the emerging problems appear, in consequence of our review so far, may be shown in certain more formal statements to which the remainder of the paper may now be addressed.

1. There is entire justification for the distinction urged by Tönnies between what have been called in English respectively 'colonies,' 'droves,' 'schools,' 'herds,' etc., in particular cases, and 'societies.' Tönnies distinguishes between the *Gemeinschaft* and the *Gesellschaft*. The difference—to put it in my own way, from the point of view of a current psychological and biological distinction—is this, *i. e.*, between the relatively unvarying, relatively definite, and relatively unconscious organization which has its extreme instance in animal instinct, and the relatively varying, progressive, plastic and conscious organization seen in human life. I shall distinguish these types as 'companies'¹ and 'societies.'

2. The distinction just made is mainly one of matter or content, seeing that the method of interaction (*i. e.*, granting that

¹The word 'community' might be used for this, as a translation of *Gemeinschaft*; but that word has another significance in English. The term 'colony' is also inappropriate, I think, for a similar reason.

it is imitation) is substantially the same in the two types of organization.

3. The first problem is, therefore, the determination of the facts regarding the 'what' of social life. What is it that is both common to all *societies* and also capable of progressive organization in each society?

4. The assumption that imitation is the method in both colonies and societies is made on the strength of recent work of various writers. Imitation may, however, be brought to a further test in connection with the problem of matter, since after having determined the sort of matter with which we have to deal, we must then ask whether the imitative method of organization adequately explains the actual forms which this material shows. To my mind the strongest proof of the claim for imitation as type of social function is derived from its effective application after the nature of the material is determined. It thus loses the casual empirical character which social observation so often shows, and becomes wrought into what may then be called, in a figure, *social morphology*. The psychological portions of my work are devoted to a detailed exposition of the imitative development of the social consciousness.

5. Finally, the determination of phenomena as social is only possible under this two-fold requirement as to matter and method. To fail in either of these is to fail entirely; on the one side it would be like determining life by morphology alone, with no necessary exclusion of crystals and plough-shares, provided they were the right shape; or, on the other hand, by physiology alone, which would not exclude a cunningly-devised india-rubber heart or an air-pump-breathing machine, provided it worked.

§ 5. Coming, therefore, to the question of the matter, the 'what,' of social organization, I shall state a general result, and then indicate certain lines of evidence for it.

This result may be put in the form of a thesis as follows: *the matter of social organization consists of thoughts; all kinds of knowledges and informations.* And in the way of further anticipation and description of the mechanism of social organization, we may add that these thoughts or knowledges or infor-

mations, originate in the mind of the individuals of the group, as inventions, or more or less novel conceptions. At their origin, however, there is no reason for calling them social matter, since they are particular to the individual. They become social only when society—that is, the other members of the social group, or some of them—also thinks them, knows them, is informed of them. This reduces them from the individual and particular form, to a general or social form, and it is only in this form that they furnish social material. It is evident that much of this is not new; my main interest in presenting it lies in certain psychological principles by which it gets relatively new confirmation, and the resulting characterization given below of the *sort of thought* which is socially available.

§6. The general considerations upon which this opinion is based may be given in contradistinction from special lines of evidence. These general considerations will be seen to arise in connection with the general requirements of social theory as stated in the foregoing pages.

1. It is only thoughts or knowledges which are imitable in the fruitful way required by a theory of progressive social organization. It has been said by some that beliefs and desires are thus imitable. It is clear, however, to the psychologist that beliefs and desires are functions of the knowledge contents about which they arise. No belief can be induced in one individual by another except as the fact, truth, information believed is first induced. The imitator must first get the thought before he can imitate belief in the thought. So of desire. I can not desire what you do except as I think the desirable object somewhat as you do. And if it be a question of imitative propagation or reproduction from one member of a social group to another, the vehicle of such a system of reproductions must be thought or knowledge. The only other psychological alternative is to say that the imitative propagation takes place by the simple contagion of feeling and impulse. This, however, takes us back to the question already raised above, *i. e.*, the question of possible progress by society. We found that the reign of imitative feeling and impulse, whether it be by instinct or by suggestion, would make possible only the form of organization

in which fixed habit is all, and in which no accommodation, movement, progress, would take place. This we found to characterize certain animal companies in distinction from true societies.¹

2. It is only in the form of thoughts, conceptions, or inventions that new material, new 'copies for imitation,' new schemes of modified organization can come into a society at any stage of its development. This seems evident from the mere statement of it. If we ask how a new measure of legislation, a new scheme of reform, a new opinion about style, art, literature, even a new cut to our coats or a changed height of hat—how any one of these things originates, we are obliged to say that someone first thought of it. *Thought* of it, that is the important thing. Feeling and desire might have impelled to thought; urgent need may have prompted the invention; decaying modes may have made reform a matter of necessity; but with all the urgency that we may conceive, the measure, the reform, the new style, has to originate somewhere in the form of a concrete device, which society can take up and spread abroad. This particular form is then the thought of someone; and society afterwards generalizes the thought. Just how this generalizing is done by society—that is spoken of below; at this stage we may simply say that society is the 'generalizing force,' in social organization, meaning that society as such does not make inventions, nor think original thoughts, much less make progress without original thoughts or, as some teach, without thoughts at all. Assuming the new thoughts originating somewhere, it is the function of society to make them available and to give them social currency; this we may call society's *generalization*.²

Then we may say that the individual *particularizes* over

¹The biological view which considers the unit-person, as such, the material of social organization may be refuted in a word. It is as *persons* that persons come into social relationships, and the differences of persons are just in the psychological part. One physical body is as good as another before the law. The distinction between things in groups and persons in society is that there is a 'give and take' in the latter case. The object of social study is thus the 'giving and taking,' and the material is that which is 'given and taken.'

²It is really a generalization, since to be thought by minds generally each such invention must be stripped of what is peculiar and characteristic of the first individual's thought.

against society. By this we mean simply that the particular forms in which new thoughts first come, in order that they may afterwards be generalized by society, originate always in an individual's mind. Just what this amounts to—how far the individual thinks from the platform of earlier social generalization—that we can not now discuss.¹ Here we may simply say that it is the individual who thinks all the new thoughts that are thought, and thinks them first in the particular form which constitutes their novelty in contrast with what is already spread abroad in society; and that of all the individual's doings, it is his thoughts which are the socially available factors of his life. Of course there is a form of social propagation which takes its origin in the actions only of this man or that, whether any thought be discoverable in the action or not. But apart from the fact that such actions have to be thought by the imitators, however spontaneous or accidental they may have been on the part of the original actor, it is evident that this form of social origination on the side of accident, mere habit, social convention or mob action is lacking in itself of any fruitfulness in the production of new phases of social progress.²

With these general considerations in mind—which are enough in themselves to justify a closer examination of the position that thought or knowledge is the matter of social organization—we may proceed to cite two lines of evidence which support this view. One of them is drawn from the facts of the child's social development, and the other from the corresponding facts of the social and ethical man's relations to the historical institutions of society. These are the two spheres in which the consideration of the psychological factors involved in social organization would lead us to expect reliable results.

§ 7. I. Special evidence from the child's social development. The general method of the child's social development has been worked out on the basis of more or less extended observations

¹ My article on 'The Genius and his Environment' may be referred to: *Pop. Sci. Monthly*, July and Aug, 1896.

² The newer works in the psychology of crowds seem to show that these represent a disorganizing and down-grade factor rather than the reverse. I think mob-action shows a bye-product or excess-play of the principles of imitation and suggestion.

of my own and other children in my earlier volume. I may quote the conclusion briefly from that work?¹

"One of the most remarkable tendencies of the very young child in its responses to its environment is the tendency to recognize differences of personality. It responds to what I have called 'suggestions of personality.' * * * I think this distinction between persons and things, between agencies and objects, is the child's very first step toward a sense of the qualities which distinguish persons. The sense of uncertainty or lack of confidence grows stronger and stronger in its dealings with persons—an uncertainty contingent upon the moods, emotions, *nuances* of expression, and shades of treatment, of the persons around it. A person stands for a group of experiences quite unstable in its prophetic as it is in its historical meaning. This we may, for brevity of expression, assuming it to be first in order of development, call the '*projective* stage' in the growth of the personal consciousness, which is so important an element in social emotion.

"Further observation of children shows that the instrument of transition from such a '*projective*' to a subjective sense of personality is the child's active bodily self, and the method of it is the function of imitation. As a matter of fact, accommodation by actual muscular imitation does not arise in most children until about the seventh month, so utterly organic is the child before this, and so great is the impetus of its inherited instincts and tendencies. But when the organism is ripe, by reason of cerebral development, for the enlargement of its active range by new accommodations, then he begins to be dissatisfied with '*projects*,' with contemplation, and so starts on his career of imitation. And of course he imitates persons. * * * But it is only when a new kind of experience arises which we call effort—a set opposition to strain, stress, resistance, pain, an experience which arises, I think, first as imitative effort—that there comes that great line of cleavage in his experience which indicates the rise of volition, and which separates off the series now first really *subjective*. * * * The subject sense, then, is an actuating sense. What has formerly been '*projective*' now becomes '*subjective*.' The associates of other personal bodies, the attributes which make them different from things, are now attached to his own body with the further peculiarity of actuation. This we may call the *subjective* stage in the growth of the self-notion. * * * Again, it is easy to see what now happens. The child's subject sense goes out by a kind of return dialectic, which is really simply a second case of assimilation, to illuminate these other persons. The project of the earlier period is now lighted up, claimed, clothed on with the raiment of self-hood, by analogy with the subjective. The projective becomes *ejective*; that is, other people's bodies, says the child to himself, have experiences *in them* such as mine has. They are also *me's*: let them be assimilated to my *me-copy*. This is the third stage; the *ejective*, or '*social*' self, is born.

"The *ego* and the *alter* are thus born together. Both are crude and unreflective, largely organic, an aggregate of sensations, prime among which are efforts, pushes, strains, physical pleasures and pains. And the two get purified and clarified together by this twofold reaction between project and subject, and between subject and eject. My sense of myself grows by imitation of you, and my sense of yourself grows in terms of my sense of myself. But *ego* and *alter*

¹ A similar view has also been reached by Professor Josiah Royce in various publications.

are thus essentially social; each is a *socius*, and each is an imitative creation. So for a long time the child's sense of self includes too much. The circumference of the notion is too wide. It includes the infant's mother, and little brother, and nurse, in a literal sense; for they are what he thinks of and aims to act like by imitation, when he thinks of himself. To be separated from his mother is to lose a part of himself, as much so as to be separated from a hand or foot. And he is dependent for his growth directly upon these suggestions which come in for imitation from his personal *milieu*."

§ 8. A further development of this with a view of determining something about the 'Genesis of Social Interests'¹ appears to bear out the conclusion that this so-called 'dialectic of personal growth,' whereby the child comes to a knowledge of himself, only by building up a sense of his social environment, may also be looked at from the side of social organization.

If we grant that the thought of self takes its rise as a gradual achievement on the part of the child by means of his constant experience of the personalities about him, and that he has not two different thoughts for himself and the other—the *ego* and the *alter*—but one thought common in the main for both²; then it becomes just as impossible to construe the social factor, the organized relationships between him and others, without taking account of his and their thoughts of self, as it is to construe the thought of self without taking account of the social relationships. The thought of self arises directly out of certain given social situations; indeed it is the form which these actual social relationships take on in the organization of a new personal experience. The ego of which he thinks at any time is not the isolated-and-in-his-body-alone-situated abstraction which our theories of personality usually lead us to believe. It is rather a sense of a network of relationships among you, me, and the others, in which certain necessities of pungent feeling, active life, and concrete thought require that I throw the emphasis on one pole sometimes, calling it me; and on the other pole sometimes, calling it you or him. But the social meaning of this state of things comes out when we look into its psychological presuppositions in the whole group. Let us then call the child's sense of the entire

¹ Art. in *The Monist*, Apl. 1897.

² This common or general part being, I think, a felt motor attitude (cf. my *Ment. Development*, p. 330).

personal situation in which he finds himself at any time in his thought, his *self-thought-situation*.¹

Now, whatever is true of one individual's growth by imitative appropriation of personal material is true of all; and we have the giver turned into the taker and the taker into the giver everywhere. The growing sense of a 'self-thought-situation' in each is, *just to the extent that the social bonds are intimate and intrinsic, the same for all*. The possibility of coöperation—as, for example, the detailed coöperations of children's games—depends upon this essential sameness of the personal thoughts of the whole circle in each situation. My action depends upon my understanding of your thought and his, and your action depends upon your understanding of my thought and his, and so on.² Looked at objectively, we say that the children are in social relationship; looked at subjectively, the truth is that they are thinking the same thoughts of the personal-social situation, and this thought is just the 'self-thought' in the stage of development which it has reached in this little mind or that and brought out on this or that occasion. H understands E in terms of her own motives, desires, tendencies, likes and dislikes, and, acting on this understanding, finds that it works; so E treats her self-thought as true to H's thought, and it works; to find that either of these expectations did not work in the great run of cases of action would be to say objectively that the social relationship was dissolved. But this could not be without at the same time disintegrating, so far as the factors were intrinsic, the sense of personal self in each of the children, or taking it back toward the beginning of its development.

§ 9. The question of the material of social organization comes up here as soon as we ask what it is that the children pass about, give and take, in this inter-play with one another. And we find here just the distinction which occurred from the

¹This phrase, which I use simply for shorthand, may be expanded *always* into: 'the social situation implicated in the thought of self.'

²In the nursery we may frequently see one child using this sameness of personal attitude for purposes of acute manipulation and childish intrigue. My child H (at 6 years) would put a high vocal value on something she did not want, and so lead E (4 years) to drop something else which H did want. H thus counted on the sameness of E's socially-induced desire and discounted it to her own private advantage.

consideration of the difference between human and animal organization. We find the child at first largely organic, instinctive, directly emotional under the influence of pleasures and pains. His sympathy is at first organic, and his antipathies likewise. But close observation shows that it is largely by the growing realization of personal distinctions, on the basis of which his thought of self develops, that he comes to have conscious imitations, original interpretations, hesitations, inhibitions, volitions. At first the relation is one of direct stimulation and direct response. If this state of things continued men would form 'companies,' not 'societies.' Direct suggestion, emotional reaction, as much coöperation as heredity might give consistently with the other features—that would be the state of things. But now let the child begin to think, and we find certain great features of social import springing up in his life. First, a distinction in the elements of his environment according as they are personal or not; second, a difference of attitude toward persons and toward different persons, according as the elements of personal suggestion which he gets will assimilate to this group of experiences or to that; third, the interpretation of the other persons in the same terms as himself, *i. e.*, as having attitudes like his in similar circumstances, and as thinking of him as he thinks of them. But all this is due to thought, involves knowledges, and the sorting of them out. The emotions now spring from thought experiences, and the attitudes, actions, responses now take on the character of means to a personal end, the end being the thought which issues in this or that attitude or action.

We may say then, as a first gain, from the consideration of the children, that what we call *objective social relationships* are the objective manifestations to the onlooker of a common self-thought-situation in the different individuals, together with the movements of its growth in each as the immediate situation calls it out.

§ 10. II. The next point offered in support of the position now outlined raises a question to which I attach so much importance from an historical point of view that I may take a little space to speak of the question itself before attempting its solution. In stating and criticising various theories above, there

was intentionally omitted a class of thinkers whose doctrine, disregarding differences of detail, may be described as the 'ideal' theory of social life. This theory generally proceeds by deduction and reaches a view of society from the presuppositions of idealistic philosophy. For this reason, *i. e.*, that the doctrine is so purely deductive, it has little consideration from the more scientifically disposed thinkers in this field. And this is the more the case since it is with the name of Hegel, with the Neo-Hegelians, that this type of social theory is associated.

In its broadest outlines, this philosophy makes reality identical with thought; finds consciousness, and especially self-consciousness, the 'coming-to-itself' of reality; and sees in social organization the objectivation or universalizing of the self-consciousness which first 'comes-to-itself' in the individual. The general social positions of this school seem to be these: first, the essential character of reality, as thought, is not lost in the objectifying whereby the individual becomes universalized in society; and second, the complete 'coming-to-itself' of reality, in society as in the individual, is in the form of a self. When we put these two positions together, we have the view that it is in the individual's formal thought of self that there is realized both the subjective form of reality and its objective form as actually existing in society.¹

It is in this conclusion rather than in the metaphysics which lies back of it—and I wish to draw a sharp line between them—that our present interest lies. This statement regarding the *thought of self* it is which our detailed inductive investigation both of the child's development and of the movements of society seems to support. This will appear from the consideration of an aspect both of the thought of self and of real social organization which I may call *Publicity*. This it remains to bring out.

§ II. We have already found so much justification for two positions: first, that the material of social organization must be considered as being thoughts which arise in individual minds and are then rethought by others, and so carried on through a social career; and second, that the child's social

¹ Hegel's distinction between 'subjective mind' and 'objective spirit.'

sense, that is his sense of all social situations, however meagre and contracted or however full and rich, arises and grows as a function of his thought of himself. In other words society to the child—society from the private subjective point of view—is a concrete situation involving related changes among the elements and attitudes which constitute his self-thought. The further question remains: given this objective social material—thought—and given also this subjective sense of society in the individual, *what is the objective character of social organization?* For, of course, the question of science is just this objective question; not only what does each individual think of the social situation when he thinks of it at all, but what must the observer think of it after he finds out scientifically all about it? His question then, in view of the two earlier determinations, is this: is the thought which constitutes the material of social organization any thought at random, thought X, thought Y, thought Z, these and others? Or must it be some particular sort of thought? And again, if the latter, must it be the sort of thought which the individual thinks when he reaches his sense of social situations as functions of his thought of himself? To come right to the conclusion, I think the last is true; and its truth appears again in what is called the *Publicity* of all social truth. What then is this Publicity? It may be gathered from this statement (which is illustrated and explained below): *every social thought implies a public 'self-thought-situation' which is strictly analogous in its rise and progress to the 'self-thought-situation' of the individual member of society.*

We may take an illustration from the ordinary attitude which society takes toward human life, in contrast with the attitude which the individual might sometimes think himself justified in taking toward his own life. Let us say that there is a question in the mind of Mr. A., as to whether he shall put a barrier across his hay field to protect himself from injury at the point at which a railroad crosses the field. He says to himself "I have crossed that field many times; I have never been struck by a train; the chances are that I never shall be; it would be useless trouble and expense." So he takes the risk of his life, and is probably justified in doing so by the event. So the sanctions

of a private kind, including that of his intelligence, would sustain him in this decision.

But now let us suppose that Mr. A is also a public official and has to consider the question of putting up barriers at railway crossings generally. He is then told that at each place at which a railway crosses a road, a certain proportion of the pedestrians who go that way are killed each year. He might say of each of these what he had before said of himself, that the chances were in favor of safety. But now that he takes a *public* point of view this is no longer sanctioned in his thought. It is no longer the question of the continuance of the life of this one man or that. It is now the question of the greatest possible safety to the collective or entire life of the community. To put up barriers at all the crossings would undoubtedly prevent the loss of many citizens a year. The social or public sanction, then, impels him in just the opposite direction; and he not only votes for the barriers, but bears a share of the taxation and *allows the barrier to be put up in his own hay field.*

If now we take this situation at its lowest terms and attempt to analyse it we find that it implies certain things:

1. A shifting of the individual's point of view, in such a way that the early private thought of self is held in check before a higher or ideal thought of self. The self of the man acting in public is different; if he be true to it, he can no longer act out his private thought. 2. There is in his mind a sense of the *reciprocity of action of all the individuals* with reference to one another under this larger thought of self.

This sense of reciprocity follows from the doctrine which we have found it necessary to hold, of the *unity of the self-content*, in all its development. We found that the *ego* and the *alter* were in great part identical, especially the part which constitutes them selves as opposed to mere bodies. We found then that when I think of myself I *ipso facto* think of you, and that the sense or emotion which the thought arouses, and in view of which I take the active attitudes that I do, reflect that thought no matter which the real *ego* may be as determined by the external conditions. But differences of attitudes arise in regular circumstances, accord-

ing as this one self-thought is imitative, aggressive, etc. Having gone so far, the very necessity of making further use of society it is which leads the child on to the further step which I have called the growth of a general or ideal sense of self. This means the formation of a category of action which includes *the essential content of self as represented* by all his earlier partial thoughts. He thinks of self as independent of the private objective marks of individuality, bodies, locality, etc. To this thought all personal actions must conform; but the actual relationships of the two selves called *ego* and *alter* must still appear in the concrete situations into which this higher thought is brought. The higher thought of self is brought to judge the lower thoughts. But it is itself a function of the lower; it could not arise except for the unity of content which holds the two together. So the result of the assimilation, the actual attitude taken in any particular case toward one or other of the lower self-thoughts—the attitude which constitutes the sense of ethical well or ill-desert as well as social value—this is identically the same attitude toward each of the partial selves. I condemn the act of you as well as the same act of me; approve it, no matter whether it be objectively determined in a particular case as really mine or yours. And this reciprocal phase of the assimilation necessarily carries the judgment over upon all the possible other people whose *ego* the identical thought may stand for. This then brings in the ejective thought of *you as also reaching the same sense of approval or disapproval that I do*. Or, in other words, the thought that the judgment passed is *actually in the minds of all other men*.

This may be put in a different way. My thought of the ideal self is general; it must sustain relation to all the particular cases. Whatever mental movement it gives rise to—approval, disapproval—must be present in all the particular cases. I find it giving rise to a feeling of condemnation in my own case when a certain action is before me. It must give rise to the same condemnation of others by me and of me by each of them. But it is said, this is very different from saying that I must think that it is actually present to them. Certainly, but we must remember that I cannot think of myself with any self

situation before me without in the act thinking ejectively on the same content; hence to think of myself, with this case before me, is to think of other men also with this case before them. To fall short of this is to think not in terms of the general thought of self, not with reference to the ideal, but in reference to some particular partial self to whose knowledge the case before me is confined.

If this is so then in the case in which I am conscious that no one but myself knows the act which I am committing, this consciousness contradicts a real element in the mental psychosis which arouses public and ethical sentiment; and as long as I fully assure myself of this, I cannot get a completely social or moral judgment. Of course it is impossible to maintain such a private state of mind in its purity; the drift toward the general statement of the case in social terms tends to establish the proper ethical sense, and imagination supplies the needed elements by whispering what my friends would say if they knew my conduct.

This means that when I think of this ideal, that is, when I bring a given action to the test of assimilation to it—for I cannot think of it in any circumstances which do not call for its application to a concrete case of action—a part of the content of my thought is necessarily the thought that the judgment is one of social generality, that others are making the same assimilation of this act to the same ideal. In case, then, I know that the action is quite private, quite secret, absolutely unknown to anybody else, then the full reinstatement of the conditions of a social and ethical judgment are *ipso facto* not present. My ideal category of action is not brought out; for to bring it out requires the very sense of reciprocity which my knowledge of privacy contradicts. If this be true to psychology, then it is no wonder that privacy destroys much of our ethical competence.

In brief we find that the 'ejective' self is incorporated in the very body of every concrete social thought since the 'self-thought situation' in the individual cannot be constituted without it. This is the essential truth in so-called 'publicity.'

3. It follows directly that it is only through the construction of a general thought of self that this publicity can be reached. For the public or reciprocal reference of the judgment in each

case arises only through the assimilation of the private and ejective self-thoughts in a larger whole of the same kind. The constituting of the larger self is just the evidence of the integrating of the more partial selves; and if the public reference is due to the common element in the different individuals' self-thoughts, then each individual must get the growth which the assimilation represents, and *all the individuals must construct somewhat the same ideal*. The former is secured in the normal growth of the self-thought-situation in each, and the latter *through their actual life in a common social tradition and heritage*.

Taking the point of view of society, further, in contrast with that of the individual, we find the state of things which social science is lead to recognize, *i. e., an actual integration of individuals just through the identical higher self which their life together makes it possible for them to set up*. From this point of view, therefore, we may call this a public 'self-thought-situation,' (expanded: *a social situation implicated in a public self-thought*) and go on to enquire into the laws of progress and development which it shows, always with reference to the individuals of whose growth it is a function. It is interesting to note that in this public self thus understood, we have reached in some degree a genetic justification of a position taken up by Aristotle and so often reasserted in the history of ethical discussion: the position which finds itself obliged to fall back upon a hypothetical 'best man' or oracle, whose judgment would be correct if it could be had. In our development, however, this public self is the objective form of organization into which growing personalities normally fall.

§ 12. But it may be said, surely it is not necessary that all thoughts, inventions, schemes, ideas, reforms, etc., should have this quality which we have called 'publicity' in order to be available for the instruction or reforming of society. Yes, they should; and that is just the point which I wish most to urge. No knowledge, simply as knowledge, can be social knowledge or become the instrument of social advance until it be made over to the public self by becoming in the minds of the individuals who think it *a public thing*, in contradistinction to the private

thoughts which they entertain simply as individuals. Whatever is thought, however great the invention, however pregnant the suggestion of reform, it is not of social value until, just by thinking it, I also attribute it to the ideal self whose entertainment of it gives it validity and general authority to all the other individuals of the group. I may, from my private judgment, discount this further development of my thought beforehand; that is, I may confidently expect that my invention will be ratified by the general thought and so come to have the requisite publicity; but I then only do so as I appeal just to that higher self already formed in my breast through social experience, and through it anticipate the fate of the thought which I thus value. But this is when the invention is looked at subjectively. As soon as we look at it objectively—that is from the point of view of the science of social organization—we have to say that no thought is social or socially available which is still in the mind of an individual awaiting that generalization by the public which will give it the character of publicity by reason of the essential attribution of it to a public self.

In other words, my private thought, in order to be social matter, must enter into that organization or integration of the public self-thought-situation which is reflected in every adult more or less adequately; it is thus thought also by that higher self in each which imposes law upon all. With this goes the thought by me that all men agree with me in thinking it, and that they will give the enforcement of it the same recognition (including its enforcement upon me) that I give it (including its enforcement upon them). The thought thus becomes involved in the growth of the personal self and just by this becomes public also. Without this connection it cannot be social. *The ultimate subjective criterion of social thought is the self-thought*, with all its wealth of implication as to the social situation. And *the ultimate objective criterion is the actual ratification of the thought by the individuals through common action upon the situation which they mutually recognize*. By this they show then common integration in a public 'self-thought-situation.'

§ 13. We come therefore in closing in upon our question as last stated to see that the growing 'self-thought-situation'

in the mind of the individual is, when viewed in its mutual interactions and correlations in the group, just the material of social organization itself; for nowhere else can we find the requisites for public availability fulfilled. Thus arises *ipso facto* a public 'self-thought-situation;' on no other view can we account for the response of individuals to the organization which society shows. So both from the side of the child's and man's growth, and from the side of society considered objectively, we are led to identify the organization of the individual's personality directly with that of society, in respect both to its material and to its method of acting. This may be made a little clearer by a short criticism of two views which are on the surface similar in conclusion to this; I refer to that of Adam Smith on the one hand, and that of Hegel on the other hand.

§ 14. Adam Smith's wonderful treatment of the social bond under the term sympathy is familiar to all students of English ethics. The criticism which I wish to make upon it is that he assumes the 'publicity' requisite to social organization, and rests satisfied with that assumption. According to Adam Smith I sympathize with what I find 'suitable' in the affections of others since it would be what I myself should experience, and the sense of this agreement is moral approbation. Then transferred to myself, my judgment of myself is a reflex of my sense of your corresponding sympathy with me. But as soon as we come to a social situation as such, that is to a situation involving two persons, an aggressor and an addressee, the question arises with which I shall sympathize. And the same question arises as soon as I come to ask about my own self-approbation or disapprobation, considered as a reflex of the sympathy of others with me. For I do not know whether the other will sympathize with, *i. e.*, approve of, me or the other whom my action affects. What then is the general element which will give publicity and constancy of value to a social action as such? This Adam Smith answers in a general way by saying that that action is approved which is most sympathized with, say as between the aggressor and addressee. But this of course does not help matters; for how am I to know which of the two you sympathize with the more, except as I again ask myself which would call

out the more sympathy in my own case. That is, the measure—strictly construing the doctrine—would after all be just what we started with, the individual's private sympathy. Adam Smith later on calls in the recognition of the judgment of a hypothetical best man to whom tacit appeal is made. But this seems to me to be simply an assumption to which he had no right; it certainly does not follow from the play of sympathies as he has depicted it.

The doctrine of Hegel on the other hand also makes the assumption of publicity. Metaphysically it makes this assumption from the start; finding just the coming of the individual to personal self-consciousness a manifestation of the universal self all the while implicit in nature. But in taking on individual form in the first stages of the realization of a self—genetically considered—it has temporarily lost this attribute; that it should get it again is to be expected; that social life is the essential stimulus to its getting it again, is *a priori* probable; and Hegel says that social life is in fact the realization of this expectation. Yet how? That is a question of fact.

Hegel's answer is, in its general character, allied to the view spoken of above as that of 'constraint.' To him the earliest fruitful social relation is that of subjection, master and slave. And its fruitfulness is, it seems, mainly for the slave, since he is domineered over, and so made to realize definite social situations. Thus certain regular self-limitations, mutual relationships, necessities of life and intercourse grow up which have the quality of general or public value when recognized by all.

This, I am aware, is a meagre enough statement of this development in Hegel's 'Phenomenology of Mind,' but Wallace's exposition may also be referred to as confirming its essential accuracy.¹ What is lacking is just the bridge from the private

¹ Professor Royce, who has kindly read this paper, thinks indeed that this statement regarding Hegel should be supplemented by reference to the functions of the family and state as described in Hegel's *Encyclopädie*. As Professor Royce agrees, however, that 'an express recognition of the imitative factor as such is what I miss in him' (Hegel), and that is my main point of criticism, I allow the passage to stand subject to later revision. I may acknowledge gratefully here other suggestions made by Professor Royce, which are to be more adequately recognized in my book.

thought to the public thought; this the imitative factor supplies. Given complex social situations, whence their validity for all the members equally, and whence the intrinsic element of public reference which is a necessity of social nature to us all? Hegel's metaphysics of course supplies this element; it is the nature of thought to recover or recognize itself as universal (*Anerkennung*) on this higher plane of social self-consciousness. But this, when scanned from the point of view of actual genetic growth, requires an empirical method of development both in the individual and in society.

§ 15. The evidence for the general conclusion now stated, drawn from the actual facts of social life, takes on many phases, and I have no space to develop it here. I may, however, note certain directions in which we may look for its confirmation.

1. Much of the matter accumulated by the great succession of English moralists to prove that sympathy in all its manifestations is a 'putting of oneself in another's shoes' is directly available. For we only have to substitute *imitative identity of the ego and the alter* for the artificial putting of one into the shoes of the other, and the results follow. This is to say that the old doctrine of sympathy is essentially correct as far as it goes, and it only needs supplementing by investigations into the genesis and nature of the class of phenomena covered by the term sympathy. This the view does which makes the self-thought a progressive imitative outcome with that play between the successive poles of its realization which is just the method of its growth. Thus a certain unity and lack of assumption is secured to the whole scheme. For example, one might take the fine catalogue of arguments given by Adam Smith at the beginning of his 'Moral Sentiments' and go over them one by one, finding that they all fall together on this view and support a derivation of publicity, where he could only assume it. For he assumes, first, that we sympathize with each other; this he makes his platform. And then he assumes that it is pleasant to both the parties when they are in a state of sympathy. Both positions are true as facts, and true also of animals. But the reason of the facts, lying in the identity of a progressive thought which just by its growth in each, integrates all in social

relationships, this is wanting. Both of these facts further are accounted for in man, by the view that from the first the gathering self-thought grows up by imitative suggestion. For on this view sympathy is a necessary motor attitude flowing from the identical thought of self; and the pleasure of mutual sympathy and coöperation is the pleasure of personal activity which is normally interwoven in a situation understood and appealed to by all the individuals.

2. We may cite the evidence which goes to show that each person does depend upon social stimulation in his personal growth, and does arrive at standards of social judgment and feeling which reflect in the main the standards current in his environment. Here the writings of Leslie Stephen, Höffding, S. Alexander, etc., may be utilized.

3. A farther interesting argument may be drawn from the statement of the same question in reference to ethical publicity, *i. e.*, the evidence which goes to show that genetically social suggestion and social beliefs are intrinsic to morality. This point is mentioned again below where the connection between ethical and social progress is indicated.

4. Finally, then is the evidence from the history of the social life of man, showing the constant 'give and take' between the individual and society which the position now taken would require.

The elaboration of any of these arguments is beyond the range of the present paper. The two last suggested lead us, however, to our final topic, *i. e.*, the consideration of the sort of doctrine of social progress we should have to hold if, as I have claimed, the matter of social organization is thought which has the attribute of publicity springing from its attribution in the mind of the social thinker to a common self, and that the method or type of function in social organization is all the while imitation.

§ 16. It has been intimated already that there are two opposed or contrasted functions in the progress of the thoughts which are socially available, seen respectively in the 'particularizing' done by the individual, and the 'generalizing' done by society. Both of these go on together, and give rise to the

conditions which social life in all its complexity presents. We may call the individual then the *particularizing social force*; he invents, constructs, interprets, on the basis of the matter already current in society and administered to him through 'social heredity.' And society, as already organized, may be called the *generalizing social force*; it reduces, generalizes the inventions of the individual by integrating them in the public 'self-thought-situation' now described. The further question then arises: how and in what direction is social progress determined under the interplay of these two types of social force?

We are shut up, I think, to a very definite view of the determination of social progress as soon as we look into the implications of the positions already taken. The positions which immediately concern us now are three: 1. Individuals can particularize only on the basis of earlier generalizations of society. This gives an initial trend to the thought variations which are available for social use.¹ 2. Society in its new acquisitions is absolutely dependent upon the new thoughts, particularizations, of individuals, and it again generalizes them. It can get material from no other source. 3. Only when both these conditions are fulfilled—when old social matter is particularized by an individual and then again generalized by society—can new accretions be made to the social content and progress be secured to the organization as a whole. Looking at these requirements together, and attempting to discover what sort of a general movement will result we find what may be called the 'Dialectic of Social Growth,' an expression which is intended to suggest a contrast with the 'Dialectic of Personal Growth,' already described above.

§ 17. In the dialectic of personal growth we saw the development of self-consciousness proceeding by a two-fold relation of 'give and take' between the individual and his social fellows. Personal material, coming in the shape of suggestions from the environment, is first 'projective' as we called it; then it is taken over into the private circle of the inner life by imitation, and so becomes personal or 'subjective,' as belonging to the *ego*; and

¹This has been developed in my article on 'The Genius and his Environment,' *Pop. Sci. Monthly*, July-August, 1896.

then again by a return movement between the same two poles, also imitative in its nature, the characters of the subject are ejected out into the *alter* personalities, so becoming 'ejective.'

The various stages into which consciousness grows—becoming social, ethical, etc.—by this one method of social give and take cannot be dwelt upon here; but it is interesting to see that this way of growing on the part of the individual consciousness may be stated in terms which reproduce in a very precise analogy the three requirements which we now find it necessary to lay down as characteristic of the growth of society. We may say, (1) that the individual reaches new inventions, interpretations, particularizations, *in his own personal growth* only on the basis of what he already understands of personality; that is of what he has learned. Each step of his progress in understanding personality is a particularization of old material in his own thought, a personal interpretation, subjective in its character. And (2) only those particularizations, interpretations, inventions, thoughts of personality, are permanently available for his growth which he again ejects outward and finds to hold generally of others also; these are generalized as habits and stand as accretions to his growth. This last is also imitative, since only the imitable elements of his subjective thought are thus true and available in his treatment of others. (3) His self-thought-situation, grows only when both these phases are accomplished together.

Here then is personal growth quite accurately stated in the same terms as those which give the outcome of our detailed examination of social progress. I am not willing to leap to metaphysical or even logical conclusions on the basis of this analogy, striking as it seems to be. But we may at least use it as an analogy, and see the further bearings of it in the matter of the determination of social progress.

Coming to make out the analogy in more detail, we see that society stands in a sense as a *quasi-personality* under a two-fold relation of give and take to the individuals who make up the social group. It is related to these individuals in two ways: first, as having become what it is by the absorption of the thoughts, struggles, sentiments, coöperations, etc., of individuals; and second, as

itself finding its new lessons in personal (now social) growth in the new achievements of individuals. If we take any lesson which society learns—any one thought which it adopts and makes a part of its organized content—we may trace the passage of this thought or element through the two poles of the dialectic of social growth, just as we can also trace the elements of personal suggestion in the case of the simpler dialectic of the individual's growth. The new thought is 'projective' to society as long as it exists in the individual's mind only; it becomes 'subjective' to society when society has generalized it and embodied it in some one of the institutions which are a part of her intimate organization; and then finally society makes it 'ejective' by requiring, by all her pedagogical, civil and religious sanctions, that each individual, class, or subordinate group which claims a share in her corporate life, shall realize it and live up to it.

Society, in other words, makes her peculiarizations, inventions, interpretations, through the individual man, just as the individual makes his through the *alter* individual who gives him his suggestion; and then society makes her generalizations by setting the results thus reached to work again for herself in the form of institutions, etc., just as the individual sets out for social confirmation and for conduct, the interpretations which he has reached. The growth of society is therefore a growth *in a sort of self-consciousness—an awareness of itself*—expressed in the general ways of action, feeling, etc., embodied in its institutions; and the individual gets his growth in self-consciousness in a way which shows by a sort of recapitulation this two-fold movement of society. So the method of growth in the two cases—what has been called the 'dialectic'—is the same.

§ 18. From these indications—which must in all cases be controlled by an appeal to fact—we see the direction in which social progress must move. *The individual moves directly toward an ethical goal.* His intellectual sanctions tend, it is true, toward a personal and egoistic use of the forces of society; but that cannot go far, since, in its extreme, it runs counter to the coöperations on the basis of which the dialectic of his personal growth as such must proceed. So with social progress. The

use of intelligence for the private manipulation of social agencies does actually represent a level of social institutional life, and in certain great departments of human intercourse—as especially the commercial—selfish ends, as seen in personal competition of wits, seems to be as high as society has yet gone. But as with individual growth so here. As soon as the personal use of the individual's wit brings him into conflict with either of these two necessary movements by which society gradually grows—or with the institutions which represent them—so soon must the individual be restrained. And, further, the restraint is no more an artificial thing, an external thing, in society than it is in the individual. The very growth of intelligence in the individual is itself a generalizing process, and by this generalization, a measure of higher restraint is set on the elements which enter into the generalization. The growth of intelligence must itself issue in those ideal states of mind which are called social and ethical and which set the direction of growth as a whole. The ethical sanctions come to replace and limit the sphere of application of the sanctions of desire and impulse; and so the individual gets, in his private life, a bent toward social co-operation and ethical conduct.

The social or communal growth shows the same tendency for the reason, altogether apart from analogy, that the actual conditions in society are the same. Society is, as we have seen, the generalizing force. It reduces the thoughts which rise and claim recognition in its midst to forms of general acceptance, and to working shape. The very institution therefore, which embodies the new idea and enforces it upon the individuals, is itself the work of the individuals, and represents the restraint of their egoistic and personal sanctions in favor of social and ethical coöperation.

Further, all the pedagogical sanctions of society, in the family, the school, etc., are brought directly to bear for the production of those social forms of habit which confirm and encourage the development of toleration, forbearance and all the virtues which are of social value.

There is, however, another and more profound reason that the direction of social progress must be determined by ethical

and religious sanctions, and toward the goal represented by a state of ideal ethical coöperation. It is to be found in the fact of what was called above the 'publicity' of all ideal thought of personality. We saw that the individual can not be a wicked or a good individual in his own opinion—that is can not get a full ethical judgment on his own acts—without, at the same time, making his thought include the similar judgment passed by his fellow men. His private self-judgment is a judgment based on the sense of a prevalent public judgment. The sense of the opinion of the public is an ingredient or element in the very synthesis by which the ethical judgment is constituted. In so far, therefore, as the growth of his personality involves a general or ideal thought of self, so far is this self a public self whose thought is *ipso facto* the birth of a sanction of a public kind. The man says to himself: "I think thus of myself; other men think thus of me; I think thus of them when they are in my place; and all for the reason that what we each and all judge with reference to, is that ideal self which each of us only partially realizes. I partially realize it in my own way, and each of the others does in his own way; and it is by these partial realizations in concrete instances alone that this ideal gets its actuality."

Now, reflection shows that social growth proceeds by just this same development. Objectively, and in fact, it is seen in the actual publicity of social institutions and interests. But the same result comes out if we take the point of view which we may call subjective to society itself. If we went so far with the analogy from the individual's growth, as to speak of society as a quasi-personality, and asked what thought such a quasi-personality would have to think in order to grow and to go on developing by the method of personal dialectic seen in the individual—we should say that society would have to think in a manner which involves the publicity attaching to ideal and ethical personality. It would have to ask what institutions were good for its citizens as such, not what was good for this particular individual or that. Its thought of personality, all the way through, would be the form of general personality, which is realized in the individuals, of course, but which is not iden-

tical with any one of them. And with this thought of general personality, there would go the thought, also, that the thought that it did thus think was the outcome of all the partial personality thoughts which the individuals thought, of all the judgments which they passed on one another; otherwise the social quasi-personality would have no content out of which to constitute its general thought of itself.

All this is simply a realization in the community, in public opinion, of the ethical standards of judgment which the individual must have if he is to develop beyond the stage of concrete egoistic or altruistic intelligence or impulsive action. That the individual does go farther is a fact; and it is just the fact which we call his development. He has attained the form of general thinking about himself and others which carries with it sentiments of a social and ethical kind. This enables him to constitute society in a way which would be impossible if he had only reached the lower development of the animals, say with the sanctions for action which go with this lower development.

So when we come to ask what the direction of social progress must be, we find that it cannot be in a direction which violates the method and denies the meaning of those very states of mind—the ideal, social and ethical states—which have enabled the individual to come into his social relationships. The ethical sanction in the individual comes to control the other sanctions, since it generalizes and so transcends them. Society represents the embodiment of these generalizations. Its institutions both represent and further the individual's growth. Its trend forward, then, must be in the line in which the individual's higher growth also proceeds. This is the trend toward the complete regulation and use of the forces of the individual *by himself* in the interests of social and ethical unity and coöperation.

Two things are accordingly true of the determination of social progress. These two things are these: first, that social progress must be determined by the generalizing agency already remarked upon working upon the thoughts of individuals; and second, that this form of determination is necessarily in the direction of the realization of ethical standards and rules of conduct.

§ 19. Finally our outcome may be gathered up in a sentence of characterization of society as a whole. Society we may say is *the form of natural organization into which ethical personalities fall in their growth*. So also on the side of the individual, we may define ethical personality as *the form of natural development into which individuals grow who live in social relationship*. The true analogy then is not that which likens society to a physiological organism, but rather that which likens it to a *psychological organization*; and the sort of psychological organization to which it is analogous to that which is found in the individual in *ideal thinking*.

SHORTER CONTRIBUTIONS AND DISCUSSIONS.

LE DANTEC'S WORK ON BIOLOGICAL DETERMINISM AND CONSCIOUS PERSONALITY.¹

The recent work of M. Le Dantec on biological determinism and conscious personality seems to me likely to perform a great service, since it expresses with remarkable precision, if I may say so, the confusion of ideas and words that has been tending to insert itself for some years into philosophical language; as a consequence this book may with advantage serve as occasion for a discussion which has really a more general range.

Let us say at the outset, to put ourselves in touch with M. Le Dantec, that this prolific author has published, within the last two or three years, a number of biological papers on the mechanism of digestion and on theories of life; he has, moreover, conceived a new theory of life, which I have already noticed in this REVIEW. His present volume is a continuation and development of the same ideas; the author endeavors to support two principal propositions: *first*, that the phenomena of consciousness have no sort of influence on material biological phenomena; and *second*, that the atoms and the molecules, not only of organic bodies, but also of inert substances, are endowed with consciousness. We shall not take up these two propositions directly, but shall discuss them as we trace the course of false ideas which the author has followed, in common with many of his contemporaries.

I wish to show, as briefly as possible, that five or six radically distinct notions have been completely confounded, and that this unfortunate confusion owes its origin to an inexact interpretation of the idea of freedom and of its contrary, determinism. Men have implicitly assumed—and that generally without being aware of the fact—that this notion of determinism is logically linked to certain other notions, and forms one with them. I shall endeavor to show the contrary, by defining anew the notions of determinism, mechanism, physiological functions, mental functions, spontaneity, and choice.

¹ Translated from the author's MS. by Professor H. C. Warren.

1. *Determinism*.—Determinism, in the very broad meaning which it received from Claude Bernard, expresses the law of universal causation; it means that there is no phenomenon without a cause, that the succession of phenomena is regular and determinate; in still other terms, it is the negative of freedom, contingency, and incoherence. Most scientists to-day admit the determinism of phenomena of the physical order; as to psychological determinism—the position opposed to that of free-will—it is accepted by some and rejected by others; the two chief arguments that are urged in opposition to it are that it compromises moral responsibility and contradicts the inner sense which every one possesses of his own free-will. I am not debating any theory now, consequently I need not take either side. To sum up, there exist two forms of determinism, *physical determinism* and *psychological determinism*; against the first is urged the doctrine of the *contingency* of the laws of nature, and against the second the doctrine of *free-will*.

2. *Mechanism*.—This is a concept radically distinct from the preceding; it is one of those which have been expressed in the greatest number of different terms, and which have consequently suffered most from equivocality. Let us, first of all, define the concept itself. In man the acts of thought, voluntary movements, etc., are of a two-fold nature; they are at once physiological phenomena, occurring in the nervous system, and phenomena of consciousness. Many philosophers have supported the idea that the phenomenon of consciousness is an epiphenomenon—something superadded, which does not intervene in the series of physiological modifications, but whose rôle is that of a passive witness. Huxley was one of the most strenuous partisans of this theory, which he expressed in several striking figures; he compares consciousness to the shadow which follows the footsteps of the traveler without affecting his progress; or to light, which may illumine the wheels of a machine without exercising the slightest influence upon its functions. In France, Ribot for some time accepted this conception and popularized it; but he afterwards rejected it.

There are, indeed, some points of contact between determinism and mechanism; nevertheless these two concepts are quite distinct. One may be a determinist, for instance, without being a mechanist; one may admit that all phenomena are subject to the law of causation, and at the same time admit that the phenomena of consciousness play an active rôle, are influenced by physical phenomena, and influence them in turn; this is the *theory of the mutual influence of the physical and the moral*, a theory which is just the opposite of mechanism.

Some authors have been either unwilling or unable to make the distinction which we point out; confusing the two concepts together, they have maintained that consciousness is a useless attribute of living matter, because, if it were otherwise and consciousness could have any influence whatever on the succession of phenomena, this would be a denial of the law of causation. An example of this is met with in Le Dantec's book, when he maintains that "everything would come to pass just the same in the world if plastic substances possessed simply their physical and chemical properties, to the exclusion of the property of consciousness"—the mechanistic theory; he seems to have been led to this theory by the deterministic theory, which he expresses as follows: "Plastic substances, like all other inert substances, are subject to the law of inertia." We will not say that this author has confused these two concepts; but if he has distinguished them he has yet established between them a bond of solidarity which he supposes to hold of itself and which he does not justify in any other way.

One word more: the theory which we call *mechanism* has been most often designated by the name of automatism. I have not used this word, and indeed I am taking care to banish it from this article, since it is a word equivocal to the last degree. It has been given, as Morgan has recently shown, five or six entirely different meanings; men have called automatic an habitual act, a non-reflexive act, an act which is unaccompanied by consciousness, an act which is conscious but necessary and determined, etc. One can never be sure of himself in using it.

3. *Physiological function*: another notion which has become singularly obscure. Let us recall first some simple facts. We do not yet know the inner nature of the material phenomena which occur in a nerve center, in its cells and its nerves, when that center becomes active; it has nevertheless been held that many of the material modifications which occur there are chemical reactions; thus for a long time it has been maintained that vision has for its starting point a photo-chemical action in the retina, and quite recently Frey has gone so far as to suppose that the simple stimulation of the tactile papillæ is propagated by means of a chemical reaction which the contact sets up in the papilla or in the neighboring cells. However this may be, there still remains an essential difference between a chemical reaction set up in a test-tube and a physiological process properly so called; the latter is composed of a series, a chain of reactions, which mutually command and influence one another—which, in a word, are *organized*. Now this notion, so simple and natural, of the physiological

process, has been greatly obscured of late; some authors have rejected it, thinking that it involved a denial of physical determinism.

It is in connection with the life and relations of micro-organisms that this confusion has come about. Here is a little infusorian swimming in a drop of water under the microscope; it goes, comes, turns about, stops beside the nutritive particles suspended in the liquid, then starts off again, changes its direction, stops, etc. Men sought to explain the movements of this little creature; it was observed that these movements are, to a certain extent, under the voluntary control of the experimenter who watches them through the microscope; he is able, by means of appropriate stimuli, such as light, the electric current and certain chemical substances, to excite the same movements, known in advance and possible to foresee as a whole, in these micro-organisms. What do these ingenious experiments of Verworn, Pfeffer and others prove? Certainly that the movements of these creatures are *determined*. But it does not follow from this that they are simply chemical reactions. Men have wrongly believed that in order to express their determinateness it was necessary to assimilate them to chemical reactions; and then, under the pen of certain scientists, the strangest analogies have been evolved: Le Dantec (*Théorie nouvelle de la Vie*, p. 32) goes so far as to compare the infusorian in movement to a piece of potassium turning about in the water which it decomposes, and pushed mechanically by a stream of hydrogen and oxygen. This extraordinary comparison is only justified by the desire to prove that nothing is left to chance in the movement of these little creatures, and that all their movements are explicable by physical causes—the deterministic position. But the deterministic position in no wise implies the conclusion that the movements in question are not physiological reactions.

Another very curious example. We know that the living bacteria contained in a preparation mass themselves at the points where a release of oxygen is taking place; the same is true of the leucocytes in the blood. Some years ago, wishing to express the constancy and necessity of the movement of bacteria towards oxygen, an eminent physiologist explained these characteristics by a chemical attraction operating between the body of the bacteria and the molecules of oxygen. This was evidently nothing but a lapse into polemics!

4. *Spontaneity*.—The notion of spontaneity is very important both in psychology and in physiology. It is contrasted with that of a stimulated or a reflex act. A reflex act is one which follows directly upon external stimulation; it is the response, or echo to it; it would

not have been produced if this external stimulation had not taken place. A spontaneous act is one which does not directly respond to external irritation; it is produced by a memory, an act of reasoning, or an internal physiological cause, as for example the circulation of the blood. From every standpoint, not only from that of science, but also from that of practice and even of law, there is the greatest interest in distinguishing between spontaneous acts and stimulated acts; the former are in general more reflective; they are more personal to their author; they carry a greater juridical and moral responsibility. This is, therefore, a useful distinction and one which should be preserved. Many authors have sought to abolish it and to condemn the use of the term *spontaneity* as being unscientific. Why? It is easy to guess. These authors have misunderstood the meaning of the word *spontaneous*; they imagine that spontaneity means first cause, the absence of determination, and that it is a synonym for *freedom*.

5. *Choice*.—This word is probably the one that has occasioned the greatest amount of equivocation. It seemed as if the faculty of choosing implied free-will, and that if this faculty were accorded to any animalcule he was thereby removed from the influences of environment, and all determinism was suppressed. The responsibility for this confusion of ideas certainly rests upon the philosophers; it is they who, in the discussion of the free-will position, have represented choice as a demonstration of that position. A falling stone does not choose, it has been said, because its fall is determined by the laws of gravitation, whereas a thinking being can choose between several different courses; this is proof of his possessing a free-will. We need not enter into this discussion. Our aim is to show that apart from every theory, laying aside that of free-will and even admitting a determinism that is universal and without exception, it is possible to give a specific meaning to the word *choice*. We may again take an example from among the micro-organisms. It has been asked whether certain species of infusoria do not exercise choice in the matter of their nutrition. There are infusorian hunters, who traverse the waters of pools with their mouths open and their cilia always in motion, and who swallow all the particles suspended in the water, drawing them towards their mouth by the current which they produce with their peristome. These creatures, then, do not exercise choice at all; provided the particle be of a suitable size it is mechanically introduced into the mouth; it reaches the plasm of the body, and there it is either assimilated or rejected, according to its nature. The problem is to discover whether other infusoria do not choose their food before swallow-

ing, that is, whether the stimulation produced by the food does not, by a reflex route, excite a movement of prehension or rejection according to the nature of the food. Observation alone can give an answer to this problem. Whichever way it be resolved, it has at least been possible to propose it, and it has been proposed without any question of free-will; for choice, thus understood, is composed of a series of regular and rigorously determined reflexes.

6. *Psychic functions.* It is here that the greatest errors have accumulated—as it were, by choice! To a number of our contemporaries, little versed in psychology—which fact does not prevent them from constructing wretched psychology without knowing it—the phenomena of consciousness present a sort of phantasy, or phantasmagoria without cause and without law. We need not reply that a phenomenon of consciousness appears to us to be as rigorously determined, in its production, its quality and its minutest details, as the fall of a stone or the budding of a plant; but it is useless to stop over this point, since these are errors which it is only necessary to clearly set forth in order to refute with the same stroke. Let us follow out the effects of this preconceived idea on the interpretation of phenomena. The question comes up again in the interpretation of the movements and acts performed by micro-organisms. Some authors wishing to endow micro-organisms with psychic properties—a disputable point, be it understood, on which only hypothesis can be made—the principal objection raised against this interpretation consists in a naïve declaration that this would mean the suppression of fixed laws. “Here is a bacterium,” says Le Dantec, “which starts off for a region of the infusion where he will find a substance which *pleases him* (!) I direct a ray of blue light upon him from another side, and he is compelled to change his route. But, it will be said, this is because he likes the light better than the food. Then I coax him in another direction by means of an attractive substance which is, however, noxious to him; he rushes up to it and dies from its effects; is this because I annoy him to such an extent that he commits suicide?”

This ironical method of reasoning is not peculiar to the author whom we cite; it may be considered as a very fair sample of the arguments of a certain number of physiologists. As regards M. Le Dantec himself, it is only necessary to notice this rather unexpected fact, that although he refuses to the infusorian, in the preceding passage, the faculty of being *pleased* with a substance, he, nevertheless, does not hesitate to accord consciousness to atoms of iron and chlorine.¹

¹ *Vide* some savory lines on the *azotic consciousness* and the *atomic consciousness*, as opposed to the *molecular consciousness*, p. 84.

But there is no need of stopping over the question whether infusoria possess any rudiments of consciousness and sensibility; in the present article I do not propose to interpret observations or to fight for a theory. I limit myself to a criticism of ideas, and from this standpoint I find that the principal objection advanced against those who wish to allow psychical faculties to the protozoa is that such psychical faculties would be the expression of arbitrariness and phantasy, that they would be incompatible with the idea that "every operation performed by a protozoan depends solely on the conditions of environment and being under determinate conditions is itself determinate."

In a word, to place well in relief the matrix-idea of all the contradictions which have been noted, we may say that there exists, even among the best minds, a tendency to admit that determinism applies less vigorously to living matter than to inert matter, to the facts of consciousness than to the facts of physics, to complex phenomena than to simple phenomena.

ALFRED BINET.

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A NEW FACTOR IN WEBER'S LAW.

Does Weber's law depend upon the real or upon the apparent stimulus? It has always been assumed that it depends upon the so-called real physical stimulus. The measurements of some illusions¹ led me to question this and investigate whether it depends upon the apparent stimulus, and if so, according to what law.

To illustrate the theory I will state its application to the results of a series of experiments made for the purpose of testing this matter in the illusion of weight, which is due to the difference in size of bodies that have the same weight. (For details in regard to the measurements of this illusion, see article cited, pp. 1-29.) The apparatus consisted of three pairs of cylinders—*A*, *B* and *C*—each of the same weight, 80 g.; the same diameter, 37 mm.; and varying in height, *A* being 20 mm., *B*, 120 mm., and *C*, 50 mm. With these I measured (1) the threshold, or least perceptible difference, and (2) the amount of illusion in *A* and *B* respectively when measured by *C* as standard.

Representing the threshold value by Δ , the results are (1) $\Delta A = 2.8$ g., $\Delta B = 4.0$ g., $\Delta C = 3.3$ g.; and (2) the illusion in *A* as measured by *C* (*K*), is an overestimation of 15.8 g., and the illusion in

¹ *Stud. Yale Psych. Lab.*, 1895, III., 1-67.

B as measured by C (K') is an underestimation of 12.1 g. These figures are averages of two complete measurements on each of twenty students of psychology who were aware of the facts and conditions of the illusion.

The constant multiples which would express Weber's law with reference to the standard, physical stimulus, 80 g., are here

$$\Delta A = \frac{1}{29}, \quad \Delta B = \frac{1}{26}, \quad \Delta C = \frac{1}{24}.$$

Now, all overestimation lowers the threshold and all underestimation raises it, and we notice, in the results, the following relations between the thresholds and the illusions:

$$\frac{\Delta A}{\Delta C} = \frac{C-K}{C}$$

and $\frac{\Delta B}{\Delta C} = \frac{C+K'}{C}$

The actual results are: for $\frac{\Delta A}{\Delta C}$, 0.85; for $\frac{C-K}{C}$, 0.80; for $\frac{\Delta B}{\Delta C}$, 1.21; for $\frac{C+K'}{C}$, 1.15. The errors involved by substituting the empirical values in the formulas are 5 % in the first and 6 % in the second. These lie well within the mean errors of observation which are: for ΔA , 43 %; for ΔB , 38 %; for ΔC , 48 %; for K , 30 %; and for K' , 41 %. Therefore, within the limits here investigated, Weber's law depends upon the apparent weight and not upon the physical standard. And, since there appears to be a definite relation between the illusion and the threshold, if the one is given the other may be calculated; for, applying the same results to the following formulas:

$$\frac{\Delta A}{C-K} = \frac{1}{23};$$

$$\frac{\Delta B}{C+K'} = \frac{1}{23};$$

and $\frac{\Delta C}{C} = \frac{1}{24};$

we obtain a constant, in this case practically $\frac{1}{23}$. Hence we may state the principle for the dependence of Weber's law upon the apparent stimulus in estimation of weight, as

$$\frac{\Delta E}{S+K} = C$$

where ΔE is the threshold, S the standard weight, K the amount of

the illusion (K will be plus or minus according as the illusion is an under or over estimation), and C a constant fraction. Hence the illusion may be used as an index to the threshold, and likewise the reverse.

Nearly all estimates of weight involve illusions, and it is probable that the above formula holds for all degrees of illusion of weight within the limits in which Weber's law is valid. It further follows from this theory that we shall find a more exact and more extensive validity for Weber's law when this fraction is taken into consideration, for most of the experiments on Weber's law have involved illusions like the one here discussed, but they have not been taken into account except by attempts to eliminate them. And, since Weber's law is a general law of all liminal sensations, we may assume, upon the basis of these experiments, that in whatever sense it has any validity it must be with reference to the apparent stimulus. A full account of these experiments will appear in *Stud. Yale Psych. Lab.*, Volume IV.

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NOTE ON THE RAPIDITY OF DREAMS.

Does association attain in dreams an altogether exceptional rapidity? Common opinion answers, yes, and appeals, for scientific support, to the records of such remarkable dreams as that of 'Maury guillotiné.' M. Victor Egger, however, in the *Revue Philosophique* for July, 1895 (40-46), subjects the evidence to searching criticism, and opposes the common opinion. He also hints at a method for investigating the question experimentally. Following this suggestion, though somewhat altering the method, I have obtained definite evidence for M. Egger's position and against the common belief.

The method is simply this: to time trains of association during normal waking conditions, count the number of scenes in such trains, and, when they are recollections, recall the time taken by the original experiences. The procedure is so simple as hardly to need description. The subject was told to begin at a signal and let his thoughts reel off as fast as they would. Sometimes he was stopped after 5 or 20 or 30 seconds, and sometimes allowed to keep on until he felt the thoughts come slowly. Immediately, he reviewed the images which had just passed through his mind, and made a mark, on a piece of paper, for each image. The 'images,' not always visual,

were required to have such a degree of separateness from the preceding and following as to be counted as separate stages of the associative process. After making this count the subject generally went on to record his reverie in detail.

The conditions of the experiment approximate closely to those of a dream recalled on waking. The main difference is that in the experiment the flow of imagination is less spontaneous, and probably, therefore, somewhat slower than in either a dream or a perfectly spontaneous reverie.

For the purpose in hand there is no need of averages or of a large number of experiments. It is sufficient to find, without looking far, frequent instances of associations rapid enough to duplicate the wonders of the famous dreams. Of the ten students on whom I experimented, one, a rather heavy, deliberate sort of man, required about three seconds for an image. Few, however, required more than half that time, and when the train of imagination was but five seconds long the time required for an image sank as low as .6, .3 or .25 seconds. Now Maury's dream, as recorded, contained not more than 16 images, and these closely grouped into four scenes. So much may easily be imagined by a man awake, in 3 or 4 seconds; and Maury's dream may have taken as much time as that.

To the objection that in dreams we certainly do live over again long series of events in a very short time, I would reply that, except for the illusion of reality in dreams, the same thing occurs in waking reveries. My slowest subject reviewed, in 110 seconds, a trip which occupied 2½ days, recalling 35 distinct and complete scenes. Another reviewed, in 37.5 seconds, a drive of three hours, recalling 19 images. Another reviewed very thoroughly a two-weeks' canoe trip, in 82 seconds, by means of 72 images. Another reviewed, in 29.5 seconds, two trips among the mountains, one occupying 4 hours, the other 20 hours. This last recollection was described as extremely full and vivid, and as comprising, around each of the 45 images, "many others of varying intensity which seem to be simultaneous." This same subject recalled, in 5 seconds, 20 images from an evening out. Still another saw, in 5 seconds, a 9-scene panorama of a trip from Boston to Detroit. Add to any of these the illusion present in a dream, and you have all that is necessary for 'living over again,' in a few moments, large segments of past experience.

I will transcribe the record of one of these experiments.

I started by looking at my table cover. Some round spots on this made me think of flecks of foam on the sea; that called up a marine

painting which I had recently seen; next I had before me in rapid succession three scenes from a rowing trip taken last summer, five scenes from a bicycle ride on the adjacent shore, and three scenes from the railroad journey thence to Boston. That reminded me of a friend whom I met on the train; and next I saw myself leaving the Boston station, loaded down with baggage, and accompanied by my friend. Soon we separated, I taking one of his cards. I then thought of some visiting cards which I had ordered and expected by mail, then of a check I had just received, of going off and spending this money, of going to Europe, of climbing the Alps. Next I seemed to be swimming across the ocean; in the middle I met a good-sized codfish, which sported with me, and finally proceeded to swallow me. I passed right through the fish's body, coming out at the tip of his tail. Grabbing him by the tail, I swung him around in the air and slapped him against the water. Flames now rose around me, generating a gas which wafted me high into the sky; there I flopped over a few times and then, diving back into the water, penetrated deeper and deeper, straight through the earth, till I emerged in the Chinese sea. There on the grassy shore stood a row of gaily dressed Chinamen, who began a lively dance, but soon changed to a row of Chinese lanterns, bobbing in the wind.

At this point I consulted my watch, and found that the whole had taken 56 seconds. As there are but 39 images, the series is not nearly so rapid as some of those of my other subjects. Add the illusion of objective reality, and we have here the conditions of a dream of 'marvellous rapidity.' "Last night," the dreamer would report, "I had a dream in which, besides minor incidents, I took a four-hours' row, a three-hours' ride, a five-hours' journey by rail, a voyage abroad and tramp among the Alps, a swim half-way across the ocean, a flying trip to heaven and a diving trip in the other direction, ending on the shores of China." And all this in 56 seconds!

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PSYCHOLOGICAL LITERATURE.

The Will to Believe and other Essays in Popular Philosophy.

WILLIAM JAMES. Longmans, Green & Co., 1897. Pp. xiv + 332.

In this volume Professor James has collected a number of discussions in 'popular philosophy,' which for the most part were first delivered as addresses before various associations and clubs, and then published in the Reviews. The title essay on 'The Will to Believe' and the second, 'Is Life Worth Living?' are the latest and, perhaps, the best known papers in the collection; but the reader will be grateful as well for the earlier pieces which are here included and will welcome the whole to a permanent place in his library.

Besides the two essays mentioned, the third on 'The Sentiment of Rationality,' and the fourth, 'Reflex Action and Theism,' are "largely concerned in defending the legitimacy of religious faith." The next four, 'The Dilemma of Determinism,' 'The Moral Philosopher and the Moral Life,' 'Great Men and their Environment' and 'The Influence of Individuals,' deal with questions of ethics and social progress. The ninth paper, 'On Some Hegelisms,' furnishes a sharp critique of certain of the Hegelian assumptions and certain features of the 'master's' method; while the last, 'What Psychical Research has Accomplished,' gives the author's well-known views on the subject in the hope of arousing interest in the field and aid for the work.

From the psychological standpoint the chief interest of these essays is to be found in the emphasis which is laid on the emotional and volitional elements in consciousness. It is probable that many readers will dissent from the conclusions reached concerning the legitimacy of the influence of the will on faith; but few will deny the accuracy of the psychological analysis, while it is time that all should recognize the deeper psychological principle involved, the principle of the interplay of the several phases of consciousness in the genesis of the various mental phenomena. How strange the 'psychologies' of the recent past, not to speak of contemporary works, will appear to the psychologists of the future! Not only the intellectualistic theories, but our analyses of consciousness as a class, will seem often to have

ignored the interconnections of the several kinds of conscious processes, or at best to have failed to supply an adequate account of them though convinced of the truth of the principle of connection in general.

But the psychology of the question is merely incidental to the purpose of the book under discussion. The author's philosophical position is described in the preface (pp. VII ff.) as 'radical empiricism,' "'empiricism,' because it is contented to regard its most assured conclusions concerning matters of fact as hypotheses," and "'radical,' because it treats the doctrine of monism itself as an hypothesis" and assumes in contrast the pluralistic view of the world. On this platform Professor James advocates as a general thesis that "our passional nature not only lawfully may, but must, decide an option between propositions, whenever it is a genuine option that can not by its nature be decided on intellectual grounds; for to say, under such circumstances, 'do not decide, but leave the question open,' is itself a passional decision—just like the deciding yes or no—and is attended with the same risk of losing the truth" (p. 11); and, in special, argues the legitimacy of the religious and ethical view of the world, that is to say, of the belief in a moral order, in the freedom and responsibility of man and in the existence of God. Thus we get a philosophy of belief which, though it guards against the vagaries of unrestrained credulity (pp. x ff., 29 ff.), affirms at once the need of faith and the legitimacy of its exercise, even when intellectual demonstration is unattained or unattainable.

In spite of the originality, one might almost say, the personality, of Professor James's reasonings, the reader of recent apologetic literature will be reminded of a certain tendency toward similar conclusions on the part of many thinkers, indeed of a certain similar tendency noticeable in the spirit of the time. And if he compare with the present age those critical eras in the history of opinion with which it is unquestionably to be classed, he will remember analogous developments in many periods when, received systems having been brought into question or discarded, appeal has been taken from the impotent conclusions of the reason to the deliverances of the heart and conscience. The issue, however, in recent times has become of broader scope. It is not merely the question of personal faith, momentous though this be; or merely the legitimacy of the appeal to the heart when the head has been brought into confusion, though, for one, the reviewer is disposed to admit this, at least in part. But modern philosophy, like modern psychology, despite its lapses from grace, has been

nearing the point at which overweening intellectualism begins to yield before the perception of the truth, that practical principles as well as theoretical are to be considered in the determination of fundamental questions. The primacy of the practical reason, it is true, was succeeded by a tremendous assertion of the omnipotence of the abstract reason. Nevertheless, it is becoming clear that we shall hardly escape from the chaos in which the downfall of the *a priori* systems left us until some more inclusive synthesis than they shall be proposed, the better to satisfy both intellectual and practical needs.

Thus the questions suggested by Professor James's work involve more than a defense of faith. They lead into the heart of the problems immediately in the path of contemporary thought. In order to meet them the philosophy of the near future will need to summon all its forces. Thinkers acquainted with the present volume will eagerly look for the more systematic treatise on empiricism of which the distinguished author gives us a partial promise (p. x.).

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The Theory of Knowledge. L. T. HOBHOUSE. London, Methuen & Co.; New York, The Macmillan Co., 1896. 8vo, pp. 626.

The division of the several sciences is a matter of convenience, and one should, perhaps, not insist that an author must confine himself in a given volume to one field and avoid all trespass upon those contiguous to it; but I cannot but think it is conducive to clearness to observe certain limits with a good deal of care. Mr. Hobhouse describes his book in a sub-title as a contribution to some problems of logic and metaphysics, and in his pages the two disciplines do interpenetrate one another. Three-fourths of his book is chiefly logical and the remaining fourth almost wholly metaphysical, or, as I should prefer to call it, epistemological.

Logic, as the science of proof, can be successfully treated without leaving the plain of the common understanding and entering into those problems of reflective thought which we commonly regard as strictly philosophical. Apprehension, construction and the processes of inductive and deductive reasoning can be so treated (and Mr. Hobhouse often does so treat them) as to be clear to one who has not occupied himself with metaphysics, cares little to attack the question in what sense the external world may be regarded as external, and never raises at all the question of the validity of all knowledge or of the

nature of ultimate reality. To be sure, logical methods must obtain in treating all these problems, but they are problems which belong, I think, to a distinct and different science, to epistemology, as I should elect to use the term, and not to the science of logic. Had Mr. Hobhouse observed this distinction, I think he would have been in some places clearer than he is, and would have avoided a certain amount of polemic which appears to me not always immediately related to the matter chiefly at issue, though it is in itself interesting and acute. He would, moreover, possibly have been led to treat at greater length in a separate volume the subjects discussed at the end of this one, and such a treatise from his pen I should regard as valuable. However, he has, as it is, given us a very interesting and suggestive book, and we must not quarrel with him for rendering the meaning of the phrase 'Theory of Knowledge' so inclusive. He has sufficient precedent for extending the boundaries of the science of logic.

In his preface Mr. Hobhouse announces it as his intention to make an unprejudiced attempt to fuse what is true and valuable in the older English tradition with the newer doctrines which have become naturalized in England. One feels, however, that he is really much more in sympathy with Mill and Spencer than he is with Hegel, and one cannot help thinking that he owes most of his best work to an inspiration obtained from English sources. I regret that he has devoted so much attention to Mr. Bradley, who does not appear to me to be the most logical of writers on logic, and he has certainly not gotten his own careful and consistent habit of reasoning from this source. He cites Mr. Bradley, it is true, chiefly to disagree with him, but he still feels that he owes him much. It is clear that he differs from him widely in his conclusions.

The book is divided into three parts, as follows: (1) Data; including chapters on apprehension, memory, construction, ideas, resemblance and identity, and judgment and its validity. (2) Inference; containing a careful and detailed exposition of the methods of inductive and deductive reasoning, with an excellent chapter on explanation. (3) Knowledge; in which are discussed the nature of validity, the conception of external reality, substance, the notion of self, knowledge and reality, etc.

It is impossible in a brief review of so extended and comprehensive a work to point out in detail excellencies or to take exception to what appear defects. I should be inclined to find the latter not so much in the properly logical parts of the book as in the psychological and epistemological positions taken by the author; *e. g.*, in his

view of our direct perception of space by sight and touch; in his putting the visual and the tangible object in the *same* place, with no further analysis of the significance of the phrase; in his distinction between consciousness as a mental activity and the content with which this activity concerns itself; in his argument to prove phenomena independent of perception and existent when not perceived—an argument which does not recognize the double sense of the word 'exist' touched upon by Berkeley and emphasized by Mill; and in his endeavor to prove, in his discussion of the conception of teleology, that a thing not yet existent but which will exist may be a determining cause of an action, whereas an action performed in view of an end which, for some reason, will not be attained cannot be regarded as determined by that end.

One of the chief excellencies of the book lies in the fact that the author is careful and consistent in his statements, a virtue not always found in philosophical writers. Evidently the work is the result of much conscientious labor, and its author has that most valuable possession, a clear mind. I feel well repaid for a careful perusal of the volume, which I shall read again with equal pleasure. In these days of much dogmatism regarding the *a priori* element in knowledge, it is a pleasure to meet with a thorough-going empiricist, who endeavors to keep his feet upon solid ground in all his reasonings, and who yet has a sympathetic comprehension of the works of those with whom he disagrees. Mr. Hobhouse refuses to accept the Neo-Kantian divorce of thought from sensation, with its subsequent illogical reconciliation, but finds, in the 'given' of apprehension, both form and content, maintaining that space, time and relations of various sorts are not the creation of thought, but are perceived by the mind, in the reality apprehended, as among its aspects or elements. Even the axioms of inference themselves he traces to a source in the 'given.' He finds them implicit in all good reasoning, and holds that they prove themselves valid in the only way in which they can conceivably be proved valid, *i. e.*, in satisfactorily reducing the whole mass of facts given to us in apprehension to a consistent and orderly system. They are proved valid, as all judgments are proved valid, by their harmony with the whole body of knowledge. One need not agree with the author in every detail, to have a strong sympathy with his general attitude upon this and other topics.

G. S. F.

Geschichte des Unendlichkeitsproblems im abendländischen Denken bis Kant. JONAS COHN. Leipzig, 1896. Pp. 261.

Dr. Cohn has given us a history of the problem of the infinite characterized by German thoroughness. From Anaximander to the Neo-Platonists, from Origen to the later scholastics, from Cusanus and Copernicus to Kant, the deliverances of the greater and of many of the lesser lights in the history of speculative thought are recorded and examined. The author has reserved for a later work a theoretic discussion of the problem, clearing the way in the present volume for such a discussion. It would, however, be impossible to make the history of any problem more than a dry catalogue of opinions, without analyzing and criticizing the various positions which have been taken with regard to it; and it is not difficult to guess from Dr. Cohn's criticisms what will be the general nature of his own discussion. He will stand as champion of the notion of the continuous; he will hold to the infinite divisibility of finite spaces and times, sympathizing, however, with the Aristotelian distinction between the infinitely divisible and the infinitely divided; and he will not believe that it is impossible for a point to move from one end to the other of a finite, infinitely divisible line, without coming to the end of an endless number of positions. In other words he will think that Aristotle has answered Zeno, and that Newton has better indicated the true nature of the infinitely little than has Leibnitz, at least in his popular utterances. What he will present will be in harmony with, I will not say the mathematical thought of our day, but rather the thought of the mathematician of our day, when he occupies himself with the discussion of this problem. His book will be interesting and suggestive, I am sure, for his knowledge of what others have written is wide, and his criticisms are acute.

Nevertheless, I cannot think that Dr. Cohn will give a clear and consistent solution of the problem under discussion unless he has—to use an American metaphor not wholly unintelligible to Europeans—some card up his sleeve better than those which appear to be in his hand. He will not be misled by mere verbal ambiguities into irrelevant discussions. He clearly recognizes in the present volume the several senses in which the word infinite has been used, and, to give an example, he is not captivated by the glitter of timeless eternity. He sees clearly the true point at issue in any discussion of the infinite extent or of the infinite divisibility of space or time, but it does not appear to me that he is able satisfactorily to meet it. Let us consider for a moment the infinitely little.

The Zenonic argument to prove motion over a given finite distance impossible, on the supposition that the space to be moved through is infinitely divisible, cannot be refuted by bringing in the notion of the continuous. If a line is really infinitely divisible, a point moving along it must assume a really infinite number of positions, and must assume them successively. If the word 'infinite' really means 'endless,' the series of positions can really have no end. One faces here a flat contradiction, one which has been pointed out with much clearness by a number of thinkers quoted by Dr. Cohn, and one which has never yet been removed by those who wish to believe the line infinitely divisible.

It is no solution of the difficulty to say that the line contains potentially an infinite number of positions, but *in actu* it does not. The point actually has passed over the line, therefore, it actually has completed an endless series. Nor can we avoid the difficulty by distinguishing between what is actually true of the line itself, and what is true only of our thought about it; in other words, by saying that we may proceed in our division of a line as far as we please, and there are always new parts to distinguish, new positions to mark. For when we call a line infinitely divisible we mean, not merely that it is practically, but rather that it is theoretically, impossible to exhaust its divisions, *i. e.*, that its divisions are really infinite. It will not do to introduce without a previous examination of its content the conception of continuous motion, or if you assume all motion to be continuous, simply the conception of motion. This conception itself needs investigation. What is meant by the continuous? Have we merely cloaked our unwelcome contradiction by transferring it to this, or have we done away with it? Are we not guilty of a *petitio principii* in assuming motion to be (theoretically) possible, when this possibility is the very question at issue?

However the matter be viewed, the difficulty remains. Either the line is infinitely divisible or it is not. If it is not, continued division results (theoretically) in simple parts, and motion means a passage from part to part. If the line is infinitely divisible, a point in traversing it must take successively an endless series of positions. It must completely exhaust this series, which is, by hypothesis, inexhaustible. The moving point becomes a living contradiction, an intellectual monster.

The real solution of the problem lies, I think, in following out the suggestion of certain writers, of whose contributions to the literature of the subject Dr. Cohn speaks with some disparagement—Berkeley

and Hume. Far be it from me to defend all that these philosophers have said on the subject of mathematics. But the suggestion that any finite line, though not infinitely divisible in itself, may be regarded as such in virtue of its proxies, appears to be fruitful.

I see a short line on the paper before me. It is a certain distance from my eyes. Division of it, carried to a certain point, results in the (apparently) non-extended. If the paper be moved nearer to my eyes the (apparently) non-extended element is seen to be extended. In other words, it is *replaced* by something which is evidently extended and divisible. A similar substitution may be effected by the use of a microscope, and there appears to be no theoretical limit to the possibility of such substitutions. Common usage justifies me in calling what I now see the *same* thing I saw before. It is the same in one of the numerous senses in which the word is used. I have substituted for a given experience another experience connected with it in a certain definite way in the order of nature, and I have abundant reason to believe that any system of mathematical relations legitimately derived from the latter may safely be carried over to all possible experiences connected with the former. Such substitutions one makes instinctively, and a man may easily suppose he is still occupied with the apparently non-extended point with which he started, when he is dividing and subdividing its representative. Provided his mode of procedure is good, it matters little whether he is clearly conscious of all the elements which enter into the process or not. Similarly, it matters little whether the mathematician can tell us what he means by his infinitesimals or not, provided he uses his formulæ in such a way as to give fruitful results.

The above solution of the problem appears to me to make possible the acceptance of those things Dr. Cohn seems most anxious to retain—the notion of continuity and the idea of a potential infinite divisibility. And it makes it possible to hold to them without falling into the Zenonic contradiction of a completed infinite. It makes the mathematical point, line, and surface rather formulæ than individual things *sensualistisch genommen*. I do not think that, properly worked out, it contains anything incompatible with a proper use of the infinitesimal calculus.

It is, of course, somewhat rash to guess, on the basis of one book, what the author will incorporate in a second. Perhaps I have wrongly interpreted Dr. Cohn's position. It would be wise for the reader to peruse for himself the 'History of the Problem of Infinity,' and I hope very much that my review may induce some to do so.

Most Americans are not so situated that they have access to many of the volumes from which our author gives citations. He has done us service in bringing this material together in convenient form.

G. S. F.

UNIVERSITY OF PENNSYLVANIA.

The Logical Copula and Quantification of the Predicate. EDWARD ADAMSON. London, David Nutt, 1897. Pp. 51.

The author of this brief essay draws attention to the fact that the Copula must be regarded in compliance with the law of identity as indicating the identical existence of subject and predicate, and that in comprehension, the copula signifies internal existence reflectively, subjective existence, identical existence with *all* the attributes implied in the predicate. In extension, on the other hand, the copula signifies objective existence, distributive existence in several individuals united together and reduced to unity by the possession of one or more identical concepts or attributes, consequently it also signifies identical existence with a *part* only of the attributes implied in the predicate. This distinction the author makes as the ground for the difference in quantification according as the view is shifted from comprehension to that of extension. The essay is suggestive, but would be more satisfactory were the discussion somewhat fuller.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

Proceedings of the Society for Psychical Research, March, 1897. Appendix to Part XXXI., Vol. XII. Address by the President, WILLIAM CROOKES.

The address of Mr. Crookes contains much of interest. It is an attempt to disprove the *a priori* improbability of telepathic and kindred phenomena. He shows by analogy the possibility of there existing certain occult forces which may account for all such mysterious manifestations; for instance, he imagines a homunculus living in a corner of our world, indefinitely small, and endowed with microscopic vision; to such an one the laws of gravitation and other physical laws would seem to be violated again and again. So also, to a person of gigantic frame and organism, other laws and other conceptions of matter would necessarily obtain. And again, should we be capable of receiving sensations with increased or decreased rapidity, then, too, the time sense would be altered and a new world would have to be constructed. His conclusion is that we live in a world, only a part

of whose forces we know, and to a part only do our sense-organs respond. And these forces, of which we are not at present cognizant, may involve nothing supernatural whatsoever, and yet they may account for the alleged facts of the occult phenomena of psychical research. Mr. Crookes offers a tentative hypothesis, as follows: That ether waves of far more rapid vibrations than those of the Roentgen rays may directly affect certain brain centers sensitive to them, without the intervention of the ordinary channels of the senses, and that such rays moreover may be freed from the limitations of space, as for instance the law of inverse squares. Thought may therefore be communicated at a great distance and without the physical connections and sequences which we deem indispensable to all communication between man and man. Mr. Crookes's speculations can not rank, however, as an hypothesis. At best he establishes merely the possibility of his speculation, for he presents no facts to indicate its probability or to save it from being relegated to the sphere of bare conjecture.

JOHN GRIER HIEBEN.

PRINCETON UNIVERSITY.

A Study in Apperception. WALTER B. PILLSBURY. *Am. Jour. Psychology*, VIII., pp. 315-393. April, 1897.

In this paper Dr. Pillsbury is engaged in the praiseworthy but difficult task of throwing light upon the problem of apperception versus association. He does this in a way that is extremely suggestive; namely, by analyzing the elements involved in the reading of a word. This, of course, involves both subjective and objective factors. The former may be analyzed into six or seven factors: the association between the letters of the word; the word as a whole; the preceding word; the events of the preceding day and hour, etc. The objective factors came from the letters themselves. The general scheme of the investigation was "to determine the amount of change which might be made in an object ordinarily perceived or assimilated in a certain way without change in the character of the resultant perception or assimilation." The object to be changed is a type-written word photographed and printed on a lantern slide. This was projected upon a ground-glass screen in front of the subject. After two-tenths of a second it was cut off, and the subject recorded what he 'saw.' Comparison of this record with the word exposed furnished means of determining the value of the various objective and subjective factors in the perception of the word. The influence of the ob-

jective factors was altered by omitting a letter or by substituting another letter or by printing an 'x' over the letter and so blurring it. The subjective factors were varied largely by Professor Münsterberg's method of calling a word, associated with the one to be shown, immediately before this one was given. The other subjective factors were the accidental variations which were noted in connection with the various experiments. The nature of all these experiments is such that the results do not admit of any complete tabulation. The tables given are merely so many examples of individual experiments, and it would be impossible to draw any conclusion from them alone. They do, however, show the comparative value of the various alterations in the conditions of subject or object. The omitted letter is most often noticed, the changed letter next often, and the blurred letter is more easily overlooked. Any change in the beginning of a word is more often noticed than if the change came later in the word. The experiments made with and without an association show that the percentage of misprints overlooked is greatly increased under the influence of the association.

The general conclusions of this study should be said to follow from the experiments, not to rest upon them. The author has taken Wundt's treatment of the theory of apperception and its relation to other mental states as a standard, and he begins his paper by a very good résumé of Wundt's theory. In the statement of the general theoretical results of his experiments, Dr. Pillsbury brings his own formulation of the process of perception into sharp contrast with Wundt's theory. "Wundt reduces the process to an associative part-process of *identity* between the parts seen and letters of the correct word, and an associative part-process of *contiguity* between these letters of the correct word and those usually combined with them to form the entire word. Apperception is present only in the passive form in which the objective or mechanical factors are alone determinant. We, on the contrary, have reduced association to a very subordinate place, and find active apperception to be the truly controlling factor." The scheme of psychology here advanced is: (1) *Sensation*, the element of all cognitive states. (2) The *idea*—a complex of sensation. (3) *Association* connecting ideas. (4) *Apperception* connecting this idea with general experience. These processes are all abstractions. The first concrete conscious process is (5) *Assimilation*, or *perception*. "This includes Wundt's association synthesis, assimilation and complication, *i. e.*, all of his associative connections, as well as the apperceptive connections of apperceptive

synthesis, the concept, the greater part of which is known as judgments, and probably agglutinations also." (6) Succession associations and (7) the highest stage of all, the true judgment; the general conclusion of the paper being "that conscious processes and their connections are not so simple as is usually supposed," and "and that what are ordinarily known as the 'higher' and 'lower' processes are not different in psychological structure and mode of composition."

J. E. LOUGH.

HARVARD UNIVERSITY.

Comparative Observations on the Involuntary Movements of Adults and Children. M. A. TUCKER. *Am. Jour. Psychology*, VIII., pp. 394-404.

These observations were made upon 18-36 adults and 13-38 children, by means of a Jastrow's automatograph. The experiments are considered in connection with the investigations of Stricker, Lehmann, Féré, and Jastrow. On the whole, the results of Féré and Lehmann are substantiated.

The results of the experiments are shown in a number of tables and cuts. As a final summary we find:

"1. There is physiological tendency for the hands and arms resting in front of the body to move inward toward the median plane of the body.

"2. There is no certainty that when we see an object we tend to move toward it. We may think of it simply as an object at rest, and the idea of motion is necessary to cause movement in that direction.

"3. Involuntary muscular movements may be controlled by the influence of the sight or visual remembrance of moving objects, and the imitation of the direction of the moving stimuli is the result.

"4. Children are governed by and subject to the same laws as adults, but to a less extent.

"5. There is no sex or age difference in children, either in involuntary or controlled muscular movements."

J. E. LOUGH.

HARVARD UNIVERSITY.

Psicologia per le Scuole. GIUSEPPE SERGI. 2d edition. Milan, Fratelli Dumolard, 1895. Pp. vii + 227.

This little book, designed for the use of high schools, has, as a textbook, the merit of clearness, directness and consistency of method. The first part is purely physiological, and the physiological point of

view is maintained throughout. Psychology is, for the author, a part of biology, namely, the study of the functions of the organism in so far as they are protective. Consciousness is only a quality which, for some reason not hinted at, some of these protective processes come to have. Consciousness is not, he tells us, a mode of being or a separate phenomenon. We might at this point like to be informed what a phenomenon means, and whether the utility for self-preservation which distinguishes those physiological functions which the author calls 'psychical' depends on their conscious quality or only on their physical complication. No theoretical question, however, is sharply faced or plainly dealt with, so that the work, in spite of its superficial clearness and dogmatism, will be far from leaving a clear impression of its doctrine upon anyone who reflects. It would have been better, perhaps, to have limited the subject to physiology proper. The author would then have remained upon ground congenial to himself and the student would not have been deceived by the idea that he had traversed the subject of psychology, when he is in fact left in well-nigh total ignorance of its historical problems and essential conceptions.

G. SANTAYANA.

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VISION.

On Reciprocal Action in the Retina as studied by means of some Rotating Discs. C. S. SHERRINGTON. Jour. of Physiology, XXI., 1897, 33-34.

Luminosity and Photometry. J. B. HAYCRAFT. Jour. of Phys., XXI., 1897, 126-146.

Ueber den Einfluss des Maculapigments auf Farbengleichungen. DR. BREUER. Ztsch. f. Psych., XIII., 464-473, 1897.

The object of Sherrington's experiment is to show that contrast is a real physiological occurrence, and not simply an illusion of the judgment, by showing that it produces an effect upon the speed of alternation necessary to extinguish flicker, even under circumstances such that it is not present at all as a conscious phenomenon. A circular disc is divided up into a number of semi-circular ring-bands, 12 mm. in width, which are painted blue, black and yellow, in such a fashion that, upon rotation, there will be (1) an inner and (2) an outer blue and black half ring-band, which will fuse in each case into a steely grayish blue, but with this difference: in (1) the blue and the black

ring-bands are neither of them accentuated by contrast, because they have surfaces of the same brightness on either side of them, while in (2) the black is blacker than it should be on account of being against a bright yellow background, and the blue is brighter on account of being bordered on either side by black. The difference in brightness between the two ring-pairs is distinctly visible before rotation; and upon rotation the effect is found to obtain which was predicted, viz: a rapidity of rotation which gave 44 alternations of blue and black per second was sufficient to cause complete vision in the inner ring-band, while the outer one continued to flicker until the number of alternations per second was sixty-eight, and this in spite of the fact that the background *during rotation* was the same for both rings—the blackish yellow of the fused yellow and black borders. The grayish blue rings were also now indistinguishable in appearance. The experiment is therefore conclusive as showing that a *consciousness* of difference of background, far from being the sole cause of the contrast effect, is not essential to its production, and hence as showing that contrast is something which takes place below the region of conscious judgments; if there were any individuals who were not convinced of this fact before, they will doubtless be brought over by this ingenious arrangement. It will be noticed that Sherrington's result is in contradiction with that of Baader, mentioned in the last number of this REVIEW. He gives, in addition, a number of good experiments to show the effect of successive contrast on flickering.

Haycraft points out that it is a pity to use the same word photometry for two things which are so intrinsically different as are isochromatic photometry and heterochromatic photometry. In the one case we are measuring—by sensation, it is true, in the last instance—something which is at the same time a physical quantity (viz., the intensity of the objective light which causes the sensation); in the other we are measuring a sensation which has no counterpart in the objective world. But this is, of course, only a particular instance of the lamentable fact that language has not yet provided us with any easy means for distinguishing, in general, between objective *light* and subjective *light-sensation*, and it is another argument for making the latter compound word more common than it is now. He uses himself the word *luminosity*, by which, however, he proposes to mean, not exactly 'amount of visual sensation,' because black is a *sensation* as much as white is—namely, the sensation which is attached to the resting state of the visual apparatus—but rather the 'amount of sensory deviation from black.' This he would take as being measured

by the number of intermediate steps which can be perceived to be such between black and a given gray, for instance. This measurement he has not yet carried out for the different spectral colors, but he has determined once more the relative objective intensity of the different parts of the spectrum at the threshold of color-perception both for the dark-adapted and for the light-adapted eye, his results agreeing with those of former observers. He also applied the flicker method to determine the relative luminosity along the spectrum. No great degree of precaution against errors seems to have been taken; "having made several such observations * * * the curve was drawn."

Dr. Breuer made a direct examination of the amount of spectral light of different colors absorbed by the macula, by comparing color-equations at or near the center with those taken in a field at from three to six degrees distant. His results confirm very closely those of Sachs made upon the extracted retina. He reaches the general conclusion that, since the total amount of absorption by the yellow pigment is so very small, individual differences in this amount cannot be of very great consequence. This fact has an important bearing, of course; it follows from it that something more is necessary to the explanation of the difference between the two types of red-green blindness.

C. L. FRANKLIN.

BALTIMORE.

A Note on the Phenomena of Mescal Intoxication. HAVELOCK ELLIS. *Lancet*, June 5, 1897.

Mr Havelock Ellis has re-examined the wonderful vision-producing properties of mescal, which were first brought to notice by Prentiss, Morgan and Weir Mitchell. Mescal buttons are the fruit of *Anhalonium Lewinii*; they are eaten by the Kiowa and other Indians of New Mexico in connection with religious ceremonial. Three of the buttons were taken in three doses at intervals of an hour; an immediate effect was experienced in the relief of a headache, which had been rather serious at the beginning of the experiment, and in a consciousness of unusual energy and intellectual power. After two hours the expected visual phenomena began with a pale violet shadow floating over the pages of an open book. Objects seen peripherally were enlarged and heightened in color, and after-images were marked and persistent. Green shadows next appeared; soon afterwards vague, confused masses of color, of kaleidoscopic character, were seen with closed eyes, which presently became distinct and brilliant, while at the same time the air was filled with perfume. Later, when muscu-

lar incoördination had reached such a stage that writing was difficult, a golden tone lay over the paper, the pencil wrote in bright gold, and the hand seen in indirect vision was red. Dr. Weir Mitchell's visions were apparently much more brilliant than these, but he could see them only with closed eyes, while Mr. Ellis found it perfectly easy to see them with open eyes in a dark room, though they were less brilliant than when the eyes were closed. Insomnia persisted during the greater part of the following night, but it seemed to be less connected with the constantly shifting visions, which were always beautiful and agreeable, than with the vague alarm which was caused by a considerable degree of thoracic oppression and of auditory hyperæsthesia. The skin was hot and dry, and the knee-jerk was much exaggerated. A gas flame seemed to burn with great brilliancy and to send out waves of light which extended and contracted rhythmically in an enormously exaggerated manner. What was chiefly impressive, however, was the shadows, which came in all directions, heightened by flashes of red, green and especially violet. "The violet shadows especially reminded me of Monet's paintings, and as I gazed at them it occurred to me that mescal doubtless reproduces the same conditions of visual hyperæsthesia, or rather exhaustion, which is certainly produced in the artist by prolonged visual attention (although this point has as yet received no attention from psychologists)." These violet shadows may be conditioned by the dilatation of the pupils which always occurs in mescal intoxication, for Dobrowolsky has maintained that the erythropsia which is common after eye operations is due to the dilatation of the pupils produced by the atropine previously administered, "so that the color vision is really of the nature of an after-image due to bright light; Dobrowolsky's explanation seems to fit in accurately with my experiences under mescal." Mr. Ellis seems not to have noticed an important paper on Erythropsia by Dr. Ernst Fuchs in a late number of the *Archiv. für Ophthalmologie* (noticed in an earlier issue of this Journal). In this it is shown, with a great degree of probability, that erythropsia is in reality entoptic rod-pigment vision; after exposure to blinding snow-light, or to the excessive amount of light admitted by a widened pupil, the rod-pigment, which is usually overlooked on account of its constant presence, becomes rapidly reconstructed and hence produces for a few moments its proper color effect. In defects of nutrition it has been often noticed, first by Parinaud, that the rod-pigment is a substance which is among the earliest to suffer; hence, even without the dilatation of the pupil, an erythropsia due to this cause might be readily expected to occur in this case.

The phenomena of mescal intoxication are, according to Mr. Ellis, mainly a saturation of the specific senses, and chiefly an orgy of vision. He is convinced that all the senses are effected; there were vague dermal sensations, and a marked casual stimulation of the skin produced other sensory phenomena a heightening of the visions or an impression of sound—a fact which may throw an interesting light on the synæsthesiæ or ‘secondary sensations.’ The immediate cause of the sensory phenomena seems to have been a great and general disintegration or exhaustion of the sensory apparatus; in a slighter degree the same phenomena, even the color vision, are found in neurasthenia. The drug, it appears, is expected to have a great future as a specific in cases of neurasthenia; the homeopathists will therefore find their account in the fact that it produces, when taken in large doses, the very symptoms which it is most powerful to cure.

C. LADD FRANKLIN.

BALTIMORE, MD.

Sight, an Exposition of the Principles of Monocular and Binocular Vision. JOSEPH LE CONTE. 2d edition. New York, Appleton, 1897. Pp. 318. \$1.50.

“In this second edition I have found little to *correct*; the changes are mainly in the form of *additions*.” Of these additions probably the most important is that on astigmatism; the portions of the book on the nature of space perception and of the laws of direction and on color have been amplified.

The conspicuous merits of the first edition are retained—the ingenuity of the illustrations, the clearness of the statements and the fascinating character of the experiments described. One characteristic still remains, namely, a misunderstanding of the psychological principles involved in monocular vision; the view is essentially a physiological one, whereas most of the facts are mental ones.

At the time the first edition was written there was no special science of psychology which was recognized by the other sciences. Introspective psychology was, for various reasons, regarded by the scientists as one stage of senile dementia. Here is an illustrative quotation from Le Conte (p. 69) concerning the theories of erect vision: “First, there have been metaphysical theories characteristic of this class of thinkers. According to these, erect and inverted are purely relative terms. If all things are inverted, then nothing is inverted. There is no up and down to the soul, etc. * * * The first we put aside as being non-scientific.” Of course, this caricature resembles

the original about as much as some of the American flags that float over Swiss hotels, with $5\frac{1}{2}$ stripes and seven stars. The introspective method of psychological investigation has received complete vindication, through experimental psychology, as being the only possible one. The very theory dismissed by Professor Le Conte, *i. e.*, that there is no up or down in our visual field except through association with bodily space, is that of Helmholtz (*Physiol. Optik*, 2 ed., p. 680) and is based on a treatment of visual experiences from the standpoint of introspective psychology. It is impossible here to discuss fully Professor Le Conte's projection of impressions back along the ray line into space. The trouble arises from treating our own mental experiences as located in another person's brain. Professor Le Conte's view of erect vision is not wrong, but incomplete. The connection of the visual field (which is somewhat improperly termed the retinal field, the two being quite different affairs) with bodily space, together with certain visual experiences, gives us our ideas of the positions of objects; we know directly what up and down mean and we know nothing of our retinas or of outward projection.

The same difference of view characterizes the section on color-perception; this, in connection with physical methods of thinking about colors, leads to curious errors. 'Unplagued by any physical considerations there are seen' to be four primary colors: red, yellow, green and blue; this omits violet, which is to the eye as different from blue as yellow from red. 'In purple we see blue and red,' which is true only of those persons who have seen purple produced by mixtures of blue or violet and red. The very same persons who 'see' red and yellow in orange also 'see' yellow and blue in green (which contains neither when pure). This whole 'seeing' of primary and secondary colors and their relations is a matter of education; it is absolutely lacking in children, to whom orange is as much a primary color as red is. It was at least different in Newton, who 'saw' seven primary colors.

Another error is that concerning the fundamental colors. The psychological view of the color system as the resultant of the mixture of three *sensations*—so clearly stated by Helmholtz and König—finds no mention, although it is merely a statement of facts and empirical laws. On the other hand, the various hypotheses concerning the functions of the retina in regard to color are extensively discussed, although they are of no interest to the psychologist and are mainly speculations of rather doubtful nature.

These same objections apply, however, to nearly all physiologies

and to most psychologies; they result, as I have tried to indicate, from a departure from the introspective standpoint. Concerning the physiology of the retina we know very little; concerning the physiology of the brain we know almost nothing; whereas our *direct* knowledge of color and space is highly developed and systematized. To attempt to systematize our psychological knowledge by deductions from the physiology of the eye is only one degree less unjustified than the attempt to produce a science of psychology by speculation on the actions of brain molecules.

When Professor Le Conte comes to binocular vision his physiology leaves him and he becomes a psychologist, experimenting and explaining directly what he sees. The clearness and completeness of his explanation leave nothing to be desired. The introduction of diagrams ready for use with the stereoscope makes this section highly interesting and practical. Amid the wealth of facts stated in the briefest and clearest manner, we find a large number of the cleverest illustrations probably ever introduced into a psychological book. In fact, the whole book, in spite of objections to its point of view, is by far the best elementary exposition of the psychology and physiology of vision with which I am acquainted, which seems an odd thing to acknowledge, when we consider that the author is a professor of geology and natural history. Professor Le Conte, indeed, is one of those leaders of science who can at any time step into a new field and get more out of it than even its own specialist.

The biologist is evident not only in rich chapters in the comparative physiology of vision and on the evolution of the eye, but also in characteristic explanations of various phenomena from the evolutionary point of view. For example, speaking of the indistinctness of vision outside of the point of sharpest vision, the author says: "Now, what is the use of this arrangement? Why would it not be much better to see equally distinctly over all portions of the field of view? I believe that the existence of the central spot is necessary to fixed, *thoughtful attention*, and this again in its turn is necessary for the development of the higher faculties of the mind. In passing down the animal scale the central spot is quickly lost. It exists only in man and the higher monkeys. In the lower animals it is necessary for safety that they should see well over a very wide field. In man, on the contrary, it is much more necessary that he should be able to fix undivided attention on the thing looked at" (p. 78).

E. W. SCRIPTURE.

YALE UNIVERSITY.

Pseudoptics: The Science of Optical Illusions. A series of psychological experiments for the classroom and home. Milton Bradley Co., Springfield, Mass. \$5.00.

This series of charts and apparatus for experiments on visual illusions is especially intended to interest the general public in mental phenomena. For this purpose it is most valuable, and should be sold in many editions. The series will also be of much interest to teachers of psychology in schools, colleges and universities. We must all spend—I might say waste—a great deal of time in preparing illustrative material which it ought to be possible to buy. Much time and energy might be saved if the simpler instruments, devices and illustrative material devised by each could be used by all. It would not be amiss for the American Psychological Association to appoint a committee instructed to draw up a list of such material and the place where it could be secured. In such a list these *Pseudoptics* would stand at or near the head.

The material is placed in three boxes, each containing several portfolios. The first box illustrates illusions of length, direction, form, size and movement, including 25 experiments in all. The charts are perhaps on the average 20 cm. square, sufficiently large for demonstration in a lecture, and the illusions in most cases appear better than in the illustrations given in text-books and articles. In nearly all cases the parts are movable, and simple devices are given for rotation, etc. We have thus not only illustrations, but a series of experiments which the student can himself carry out. The second box illustrates after-images, color-mixture, contrast, indirect vision and the blind-spot—the term illusion being used in a sense wide enough to include all cases where, through the functions of the eye, nervous system or mind, we see things otherwise than as they ‘really are.’ The third box illustrates especially perspective and binocular vision.

The series is accompanied by an introduction explaining the objects and advantages of the experiments; the method for making each experiment is described in sufficient detail, and there are given explanations of the phenomena. These latter are of necessity brief, and in some cases may prove misleading, as they may cause the student to imagine that the phenomena are more simple and better understood than is in fact the case. The classification adopted may also in several cases prove misleading. Thus, for example, under ‘multiple vision’ are included phenomena so diverse as are binocular double vision and the doubling of the image in Schreiner’s experiment. The apparent similarity and real diversity in such cases may easily confuse the student.

The author of *Pseudoptics* wished originally that his name might not be associated with it. But it has been announced by a firm of instrument makers, and there is now no reason why we should not give honor where honor is due, and thank Professor Münsterberg for his valuable service to education and to psychology.

J. McKEEN CATTELL.

Ueber die Bedeutung der Convergenz- und Accomodationsbewegungen für die Tiefenwahrnehmung. MAXIMILIAN ARRER.
Philos. Studien, XIII. 1. 116-161. 2. 222-304.

The author investigates the problem of the perception of depth from the standpoint of the part played in such perception by the sensations of convergence and accommodation. In a dozen pages he reviews in concise statements the experiments and discussions on the subject in the past in so far as they involve these sensations. Chapter I. communicates the author's experiments on the perception of differences in depth by comparison of successive stimuli. It is believed that this problem, which was long ago investigated by Wundt, will bear a fresh investigation now, because of the objections which have been raised to Wundt on the grounds that his experiments are not numerous enough (this is admitted by Wundt); that the subject upon whom his experiments were carried out did not possess average capacity in the perception of depth owing to a defect in the mechanism of the eye, and that the theoretic constructions which Wundt gives to his results are not warranted. Chapter II. is an attempt at an explanation of the monocular and binocular experiments of chapter I. Chapter III. further attempts a negative confirmation of the theory of the former chapter.

The apparatus used in the first set of experiments, viz., in those on the monocular and binocular perception of differences of depth, was as follows: the subject looks through an inwardly blackened tube, which passes through a screen of black cardboard, upon a gray field. In the interval between the tube and the gray background two black threads are kept stretched perpendicularly by weights. The distances between the threads are varied by moving one of them nearer to or farther away from the other, which in turn remains unmoved during each series of experiments. After showing one thread until the subject has a clear image of its absolute distance from him, a screen is placed before the tube, the one thread is lifted up, the other is left to hang in its place in the field of vision, and the screen is re-

moved, the problem being to say whether the second thread is at the same distance, nearer or farther away than the first one. The author's results agree, in the main, with those of Wundt. They differ in that they show smaller values for the recognition of differences of depth, but the author writes that he withholds further communications for another place. The author's explanations, however, differ quite essentially from those of Wundt. According to the latter, differences of depth in the direction of the eye of the observer are recognized through the sensations accompanying the movements of accommodation to the increased nearness of the object. Differences of greater distance away from the eye, on the other hand, are recognized by differences in the thickness and clearness of the thread, the theory being that the accommodations to increased distance are brought about by simple relaxations of the accommodation-muscles, corresponding to which there are no peculiar movement sensations. The author finds that the apparent differences in the thickness and in the distinctness of the thread are far too slight to serve as the grounds of the perceptions of difference which the tables show, and he accepts changes of sensation corresponding to the accommodations to greater distances. The results of the discussion of the binocular and monocular experiments are gathered together as follows: "(1) The sensible factors in localization in depth, relative and absolute, are sensations of convergence and accommodation. (2) The estimation of depth takes place, neither through an immediate perception of the degree of effort of convergence nor through an association between these sensations and the object to be located, but simply through the fact that these sensations are the particular elements in the space-representation (*Raum-Vorstellung*), which for our consciousness condition and bring to expression the relation of depth." The perception of depth is, according to the author, an assimilation process pure and simple. The most important moment in the monocular perception of depth is the sensations of accommodation; the most important in the binocular perception of depth is the convergence sensations.

The discussion represents a very large number of experiments and a careful study of the literature of the subject. Occasionally the discussion seems to be unnecessarily lengthened, whereas the views of other writers are sometimes somewhat too briefly given. From the writings of Descartes and De la Hire down to the recent discussions of Stumpf, Lipps, Dixon and others nothing seems to have escaped the notice of the author. Yet no mention is made in the entire thesis of the theory of James and Ward, and the author simply

assumes the sensations of accommodation and convergence. In his discussion of the views of Wundt and Hering, one has the feeling that all has not been said which might be said. Those interested in the subject will in the future no doubt have to take this study into account.

G. A. TAWNEY.

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SLEEP AND DREAMS.

Expérience sur les rêves. J. MOURLY VOLD. Édition privée. (Repr. fr. Rev. de l'Hypnotisme, January, 1896.) Christiana, Actie-Bogtrykkeriet, 1896. Pp. 16.

Einige Experimente über Gesichtsbilder im Traum. J. MOURLY VOLD. Zeitsch. f. Psychologie, 1896, XIII., 66-74. (Repr. sep.)

Die physiologischen Beziehungen der Traumvorgänge. CARL MAX GIESSLER. Halle, Niemeyer, 1896. Pp. 45.

Professor Vold's two papers describe some experiments upon the muscular and optical elements entering into dreams and the conclusions to which they lead. The experiments were performed upon the author and some forty others, of both sexes and for the most part adults, who volunteered their assistance.

The first paper reports the experiments upon muscular stimulation. The author met the subjects beforehand and explained to them in detail the nature of the experiments, but without a hint as to the expected outcome. The experiments were not to be begun until the day following the interview, in order to avoid any direct influence of the latter upon them, and the subjects were requested to refrain from all exertion on the evenings of the tests. Immediately on waking in the morning after each test the subjects were to answer in writing a number of questions concerning the dreams of the past night. With most of the subjects a considerable number of tests were made. The conditions of the experiment consisted simply in confining certain sets of muscles with a glove, ribbon or string. The two hands and the tibiotarsal region were the parts especially used. The disturbing influence of the preparations for the experiment was obviated by duplicating them, in one case putting the glove on and then removing it before sleeping, the next evening keeping it on through the night. To distinguish between actual movements and sensations of movement due to the artificial muscular stimulation, the subject was asked to note carefully each time whether there were signs of his having moved just

before waking. Experiments were made alternately with the right and left sides, and sometimes with both sides together, in order to determine the relative influence of these different conditions.

Among the more important results of these experiments, the author finds that we generally tend to notice the position of a flexed limb, whose sensations enter into our dreams and form an integral part of them; we rarely dream of being in a horizontal position. The influence of the bodily position on dreams is as follows: The part which is flexed or whose muscles are confined may be represented *statically*, as being in the position in which it actually is. Or the whole body may be represented as performing a *movement* of such a character that the part in question plays a prominent rôle in the activity. Again, this same movement may be dreamed of as opposed or prevented. At other times the dream represents another person or an animal as being in the position or performing the movement. Finally, in some cases where the fingers are confined, a dream occurs in which the subject is occupied with a number which corresponds to the number of fingers affected. In this last case the connection seems rather forced, and the author must give more detailed results before his position can be accepted.

Professor Vold supposes these different effects to be due to differences in the degree of fatigue of the interested centers. When fatigue is greatest the peripheral sensation may barely reach the threshold of consciousness, giving a general notion of the number in question, without any distinct idea of its peripheral origin or of its belonging to the subject himself. When fatigue is less the notion of the limb may be more distinct, but still without a tendency to associate it with his own person. In more superficial sleep, where fatigue is slight, the subject is at length able to associate the sensation with his own body. The author considers that the active interpretation of these sensations as movements is due to a greater degree of fatigue than the static, since the latter involves a clearer consciousness of the actual condition of the limb. We cannot but think that he lays too much weight on one hypothesis, which he uses as counterpoise for his experiments, viz., that we never remember dreams in which actual movements occur.

Professor Vold's second paper, read at the Psychological Congress last year, deals with the visual elements in dreams. The subjects were each provided with a parcel containing a number of small objects or figures cut from cardboard; this parcel they opened in bed and, placing the objects upon a black or white background, observed

them fixedly for a certain length of time, generally from 2 to 10 minutes, but occasionally, with intervals of rest, for half an hour; they then extinguished the light without looking at the flame. The same method of reporting the results was used as in the muscular experiments. The success of the experiment seemed to depend upon a number of factors: the general disposition (*Anlage*) of the subject, the quiet and normal passing of the preceding evening, his health, the absence of undue fatigue, and the exact and systematic carrying out of the experimental conditions.

The results themselves, as reported, seem somewhat general and vague in character; the author limits himself to a few striking examples, and does not attempt to tabulate the experiments at all. The paper as a whole is, therefore, rather unsatisfactory to the exact scientist. The test-object, says Professor Vold, rarely enters into the dream unaltered. Its form and size may reappear with change of color, or *vice versa*, or one or more of these elements may appear transformed or become so in the course of the dream. White and black in the test-objects had the most marked influence; these would often appear in the dream under the form of simultaneous or successive contrast-effects. The test-object occasionally reappeared in the given color or its complementary, or another object would be seen in the color of the given test-object. With colors other than black and white, the given color was sometimes exactly reproduced, but oftener appeared changed as to saturation, brightness or color-tone; in some instances the complementary color appeared. Unfortunately nothing is said as to the relative frequency of these different cases, nor of the proportion of successful reproductions to the whole number of trials.

From these results the author deduces the conclusion that the visual apparatus immediately before waking reproduces to a certain extent the condition present at the beginning of sleep. The brain cells, however, work independently in sleep, and the syntheses of form, size, color and abstract representation constructed by day or in the evening are broken up; in place of these new syntheses are built up between the outlines and abstract representations of daily life, on the one hand, and the outlines and more especially the colors of objects which affect the visual apparatus just before the beginning of sleep.

In contrast with these two papers, which emphasize the psychological side, Dr. Giessler's is a contribution to the physiology of the dream processes. The author assumes at the outset that the distribution of nervous energy, which in waking life is directed into certain channels by the voluntary working of the attention, is in dreams,

through the inhibition of the higher centers, mainly passive, uncoordinated and directed without effect to various points of the nervous system.

Dream illusions are due to several causes: 1. To peripheral stimuli which fail to reach the threshold; here the stimulus may either be transformed immediately into an illusion, without any sensation of the stimulated part coming into consciousness; or it may be transformed through a mediate association. 2. To stimuli which reach the threshold discontinuously; in this case the vague sensation of the part stimulated gives rise directly to an illusion. 3. To reflexes, which do not as such come into consciousness. 4. To sensations which reach consciousness, but are subjected either to changes of quality and localization or to an increase of intensity. 5. To feelings, which in connection with the intellectual elements bring about a heightening of the emotional side. The author proposes an explanation of the underlying physiological processes in each case. In the first case, *i. e.*, where the stimuli themselves do not reach consciousness directly, he supposes that certain stimuli at some period attain a high degree of intensity; a number of such stimuli are brought into association by a subcortical process, and the coördinated product is transmitted to the appropriate cortical center; there it stimulates the traces of former similar coördinations; between these an association is brought about, which appears as the memory image of a presentation that has previously accompanied a similar bodily condition. The physiology of the other cases is similarly explained. The author illustrates each case with the example of an actual dream. In the case cited he describes a dream in which he appeared to be standing before a booth at a fair, surrounded by a crowd of laughing acquaintances; he ascribes the situation to a peculiar posture in sleep which suggested standing; the laughter was suggested by the difficulty of breathing occasioned by his posture and by other bodily feelings which he noted on awakening.

Dr. Giessler next takes up dreams of hallucinatory character; these are due, as he explains, to an *idea* of some sort, rather than direct peripheral stimulation. Thus the strong notion of something to be avoided may give rise in dreams to the experience of its actual occurrence. The physiological process here consists, first, in the concentration of nervous energy along certain already formed paths, whose mental products are capable of giving a suitable turn to the dream; and second, in the prevention of its outflow to other centers which would produce unfavorable changes in the images; the assist-

ance of the visual center is usually needed in such dreams as a support for the other centers. Hallucinatory dreams are divided into: (1) affective dreams; (2) those involving the higher mental functions; (3) the reproduction of common presentation-series.

The author discusses at considerable length the processes involved in three special cases: visual space-localization, speech and writing. The space relations are distorted in dreams in two ways. Since the muscles which raise the eye-ball offer greater resistance than those which depress it, the nervous energy which is transmitted to them gives rise to a lesser movement; the dreamer, therefore, estimates the upward movements as less in proportion than the downward, since they give rise to the feeling of a lesser outcome. As regards depth, the original position is usually estimated correctly; but when the eye moves to another point the innervation feeling of the accommodation center remains practically unchanged, and hence in dreams the distance from the eye of any two points fixated in turn is the same, or the difference seems much less than it really is in waking life. The estimate of lateral distances is not subjected to any distortion.

Dr. Giessler formulates six laws governing the production of dreams, two of which apply to the phases we have especially noticed. 1. There is a tendency in dreams to refer conditions which are caused by stimuli below the threshold of perception or above the threshold of apperception to a substratum outside of the dreamer's body, while those conditions caused by stimuli lying between these two thresholds are referred to the dreamer's own body. 2. The nervous energy sent out to a system of organs (*e. g.*, those which regulate space perception) stimulates the different parts of this system more quickly, more intensely and more definitely in proportion as they belong to an earlier epoch in the historic development of that system.

A very complete classification of dreams is given at the end of the paper, based on the nature of the mental functions involved.

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PRINCETON.

A Contribution to the Physiology of Sleep, based upon Plethysmographic Experiments. W. H. HOWELL. *Journal of Experimental Medicine.* Vol. II., No. 3, 1897.

Some twenty experiments were performed, but of this number only four or five gave entire satisfaction. Each experiment covered about four and a half hours of normal sleep. The volume changes in the hand and the lower part of the fore-arm were measured by means

of a water plethysmograph, due precautions being taken to keep the enclosed parts immovable and to secure a comfortable position. The record was inscribed upon a drum revolving once in twelve hours, and was supplemented by the notes of a watcher. Neither pulse nor respiration was registered.

An examination of two curves obtained in successful experiments shows dilatation of the arm at the beginning of sleep, the maximum being reached at one to one and a half hours and maintained for an hour or two, when constriction appears, bringing the arm, first gradually, then more rapidly, to its normal volume at awaking. Within this general course of the curve there are waves of an hourly period and sharper oscillations that are much briefer. The larger variations indicate a lowering of the peripheral resistance in the skin area with diminution of arterial pressure and of the blood flow through the brain. The periodical wave-like oscillations point to rhythmic changes in the vaso-motor center, and the shorter oscillations are due to external stimuli, deep respiration or bodily movements.

A comparison of this plethysmographic curve and the intensity curves published by other investigators or obtained by the author himself, shows a resemblance during the first period only; the deepest sleep seems to correspond with the minimal flow of blood through the brain. Beyond this period, the parallelism ceases, the irritability of the cortex returning rapidly to the normal while the anæmic condition of the brain persists for some time.

Sleep, according to the theory advanced by Professor Howell, results from the combination of three factors: "A diminution of irritability, caused by fatigue, of large portions of the cortical area; voluntary withdrawal of sensory and mental stimuli involved in the preparations for sleep; a diminished blood supply to the brain, owing to a relaxation of tone in the vaso-motor center and the fall of general arterial pressure thereby produced. The last factor is the immediate cause of sleep and explains its comparatively sudden and nearly simultaneous occurrence over the entire cortex."

As to the possible play of psychical processes no distinct information is afforded by this paper, since the sleeper did not, in any of the experiments, have a conscious recollection of dreaming. It is noteworthy, however, that in some cases there was partial awakening without permanent constriction of the arm and consequently without permanent increase of the blood-flow to the brain. In explanation, the author suggests that the metabolic processes within the cortical cells might be increased by either internal or external causes other

than changes in blood supply, and might thus rise above the threshold of consciousness. The conscious processes might then outlast the corresponding vaso-motor changes.

It may be permissible here to observe that in the much shorter sleep record published by Shields (Jour. Exp. Med., Vol. I., No. 1), odor stimulation did not affect the general direction of the curve showing increase of arm volume in the first period. Not all the odors employed were accompanied by the same change in direction or extent; nor was the action of any one odor uniform. While it would be difficult to draw satisfactory conclusions from these peculiarities, and while, as Shields has pointed out, these changes give no clear evidence of sensory reaction, it is conceivable that the effect of any stimulation is determined in some way by the condition of the vaso-motor center at the moment the stimulus is applied. As this center, according to Howell, is the seat of rhythmic changes which account for the large periodical variations in the plethysmographic curve, it would at least be interesting to observe the effects produced by stimulation in various phases of the rhythm.

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GENETIC.

First 500 Days of a Child's Life. MRS. WINFIELD S. HALL.
Child Study Monthly, November to March, 1896-7.

In five papers appearing under the above title Mrs. Hall has outlined the history of the first five hundred days of the life of her child. While less critical and less exhaustive than Miss Shinn's, where the two writers whose work can be so favorably compared have entered the same field, Mrs. Hall's observations are more completely classified, and she has not hesitated to point out a number of conclusions. Her observations have undoubtedly been carefully carried out, and this will render her history not only interesting in itself, but useful for purposes of correlation with the results of other observers. For the introductory chapter on growth we are indebted to Dr. Hall, the father of the child; but as the results of his measurements do not appear again in connection with the features of development which Mrs. Hall describes, we may pass this chapter over and go at once to the consideration of her own papers.

In the introductory outline of the classification according to which the observations are arranged, we find two main divisions of devel-

opment, physical and psychical. Under physical development are subsumed muscular movements and coördinations; while included with senses and intellect under psychical development are emotions. If the motor element is as significant in consciousness as we are coming to believe, and if visceral sensations and emotional expressions constitute the differentiae of the emotions in consciousness, this division is an unfortunate one, inasmuch as it separates these phenomena by such barriers as the terms physical and psychical would tend to produce.

In the second paper we regret to find among muscular movements records of so few inherent reactions, for without these a history of the development of muscular movements must be incomplete. For example, under 'grasping' no mention is made of a reflex, though it is recorded of the 57th day that "for the first time he seemed to know that he had something, and his fingers tightened upon it." The history of the development of grasping and of sucking the thumb is given on the whole as a history of voluntary movements, *i. e.*, attempted conscious adaptations, rather than as a history of muscular movements proper.

On page 395 Mrs. Hall describes the chance discovery of a useful movement, showing that the child, in common with young animals, may develop through the wider application of instinctive movement. There are many observations which suggest questions of interest. For example, the thumb was constantly enclosed in the fist till the 70th day. In the case of my own child the thumb was rarely enclosed in the fist. What, if any, is the significance of such differences in hand attitudes, and have they a bearing upon the subsequent development of hand movements? And we are impressed by the fact that many movements were taught to the child. Was this done in conformity to a prevalent belief that the various forms of habitual actions must be learned from another? Or was it proved experimentally in this case that the child would not or did not acquire these habits without instruction? If the last be true then there is one case to be cited in support of a popular belief. It is of importance to know whether these habitual movements can be acquired altogether without instruction or by imitation, or whether these are necessary or of assistance. Nearly all the movements described by Mrs. Hall are repeated or imitated movements, either repetitions of copies set for the child in terms of movement, or of those seen and translated by him from visual to motor terms. We should like to know whether he could reproduce a movement from a copy held in its own terms more easily, or at an earlier date, than he could reproduce one from a copy held first in the terms of another sense.

It is well to indicate the value of observing the development of coördinations, and Mrs. Hall's remarks upon this are extremely suggestive. But we feel the need of a more comprehensive history than she gives us. Here again we should know more of the inherent coördinations, and of how far imitation was influential in impressing combinations of movements upon the organism, and of how far experiment and the accidental results of chance movements tended, if at all, to modify a recognized order of development of coördinations.

The definition of coördination is open to criticism; for by the use therein of the word graceful, a number of skilled adjustments would be excluded from the list of coördinations. Many highly dextrous artisans are not graceful, and many exquisite adaptations required by the use of tools render grace of movement impossible. Later (p. 406), there is a second definition of coördination which is designed to fit a conception rather than the phenomena as they are seen to occur; for such movements as the symmetrical ones of the arms in early infancy are to be viewed as primitive coördinations, yet not as adjustments in time or of force, but rather as the preliminary steps by which data for the knowledge necessary to such adjustments are acquired.

The history of psychical development opens with observations on the senses. Among the records of vision there is little to note, but we cannot pass by the conclusions without pausing over the fourth one (p. 468). Here Mrs. Hall writes: "The time when visual perception becomes relatively clear precedes the following of moving objects by the eyes because: (a) this act is a voluntary one; and (b) the child cannot will to follow the motions of an object which it does not perceive." There is not, I believe, as yet enough evidence to establish a claim of priority for either fixation or following. Miss Shinn is of the impression that following may occur very early, and my own record shows that it may precede fixation. Nor can following be classed among voluntary movements, the evidence at hand going rather to show that its place is among the inherent ones. Under conclusion 9 the suggestive fact is noted that "attention is held much more closely when two senses are affected than when only one is affected." Among sensations we miss observations upon touch, taste and smell. When we come to the emotions and the intellect, however, we find fuller records, and this is especially true of the subdivision of intellect which treats of language. Over all of these we should like to linger, for the observations will well repay a careful analysis, and the conclusions are worthy of consideration.

One impression grows within us as we reach the conclusion of

the fifth chapter: it is that every advance is in some sense a repetition of experience. There is not a case on record in which the child took an initiative, or launched on a wholly independent line of action. When something strikingly unusual was performed, such as is recorded on page 534, or of the occasion upon which he alternately struck two objects to produce different tones, he was accidentally led into these performances by the discovery of qualities in the objects. Such a collection of records gives a natural history of the development of conscious continuity. And we must mark it as a distinct advance that Mrs. Hall has contributed a history of mental development rather than a mere record of dates.

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MENTAL FATIGUE.

Ueber die Beeinflussung einfacher psychischer Vorgänge durch körperliche und geistige Arbeit. S. BETTMANN. Psychol. Arbeiten I. Pp. 152-208.

Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit. E. AMBERG. Psychol. Arbeiten I. Pp. 300-377.

Ueber Ermüdung und Erholung. W. H. R. RIVERS and E. KRAEPELIN. Psychol. Arbeiten I. Pp. 627-678.

On Mental Fatigue and Recovery. W. H. R. RIVERS. Journ. Ment. Sci. XLII. Pp. 525-528.

Studies of Fatigue. J. M. MOORE. Stud. Yale Psychol. Laboratory III. Pp. 68-95.

Untersuchungen über die Einflüsse der Arbeitsdauer und der Arbeitspausen auf die geistige Leistungsfähigkeit der Schulkinder. J. FRIEDRICH. Ztsch. f. Psychol. XIII. Pp. 1-53.

The influence of fatigue on mental performance is the subject of these six articles. Herr Bettmann has investigated the effects of fatigue, incident to both mental and physical work, on the time of certain mental processes; Amberg has experimented upon the influence of rest periods on mental ability; Dr. Rivers and Professor Kraepelin take the general problem of recovery from fatigue; Dr. Moore has investigated the effect of fatigue upon certain voluntary movements, while Herr Friedrich has given his work a practical turn, and found the effect of the fatigue of the school day upon children's ability to do some ordinary school tasks.

1. Bettmann's article gives the results of the influence of two hours' rapid walking or of one hour's adding figures upon choice reactions, word reactions, memorizing figures, adding, and rapid reading. For the first of these processes he finds that the fatiguing mental work increased the time; average normal time 293σ, after adding 384σ. The reactions taken after the walking show a decrease in time, 257σ. This is explained by the number of false reactions apparently included here, the percentage of these under the different conditions being as follows: normal 2.6, after mental activity 1, after bodily work, 29. The bodily work as well as the mental increases the time for word reactions. Memorizing was found more difficult after the fatigue of adding, the decrease being slightly greater after the bodily work, although during all the experiments there was a 'practice' advantage for the latter. The average number of figures learned in one-half hour was: normally, 661, mental work influencing, 476, bodily work influencing, 454. The influence of the two kinds of work shows itself clearly also in the average number of figures added during one-half hour: normal, 1793; after mental work, 1572; after bodily work, 1571. The average number of syllables read normally in one-half hour was 8798; after the mental work only 7660, and after the bodily only 8380 were read.

From these results the author concludes that *Turnstunden* and *Spazieren* should not be used as means of recreation before mental work. One must remember, however, that when we walk or swim or play tennis we do not do them *in recht raschem Tempo*, and that also in our recreations there is a decided interest which must have been lacking in the two hours' march. On the whole the work is well done, and the results are fully collated, but only one observer was tested. In common with the other two *Arbeiten* articles, and with Friedrich's article in the *Zeitschrift*, the material would have made more interesting reading if it were not spread over four times the space required.

2. In this research the author attempted to determine the effects of different periods of rest, of the difference in the kind and duration of work, and of personal differences. Adding and memorizing were the mental processes used in the investigation. A rest of five minutes between two half hours of adding showed a 6 per cent. increase in the amount done over that when no pause was made. When there was a continual change, five minutes work, five minutes rest, scarcely any increase was noted. For two observers, fifteen minute rests between two half hours' work showed no effects; when the work was two

hours long and the fifteen minute rest was taken between the hours there was noted a slight increase in the amount of work done. For the author a fifteen minute rest between two half hours' memorizing gave a $6\frac{1}{2}$ per cent. decrease in amount accomplished; another observer, however, under like conditions showed an increase of 13 per cent. These rather conflicting results show the need of further and more extended work in this direction.

3. Professor Kraepelin's and Dr. Rivers' paper is a partial answer to the question: What period of rest is necessary for the recovery of mental freshness? Between the different half hours' adding of single figures a rest of a half or of a full hour was taken. The results show that for a normal man a rest of the same duration or of that of double the period of work is sufficient to restore the mental freshness once, after which there is a rapid decrease in the capabilities which cannot be balanced by a simple rest. During the work many temporary personal influences showed themselves. How far the results of the latter two researches can be extended to daily life, to all kinds of mental conditions, it is difficult to say; in all probability, the question of interest would be one of the great influences in daily work, and to draw conclusions from uninteresting, not to say wearisome, experiments as to what would happen under ordinary conditions, would be extremely hazardous.

4. Dr. Rivers' second paper only gives the method and general results of the preceding research.

5. In Dr. Moore's studies, two observers were tested as to the effect of fatigue on binocular estimate of depth and from the first to the last experiment there is a gradual increase in error of estimate. Three observers gave practically the same result in monocular estimation of depth. The time of monocular accommodation increased for one observer from .35 s. to .87 s. (296 experiments), for another observer, first series, .36 s. to .46 s. (391 experiments), second series, .30 s. to 61 s. (261 experiments). Taps were made as rapidly as possible with an electric contact key. Evidences of fatigue showed themselves at about the 70th tap. Fatigue lengthened the time of making each tap, the average for the first ten being 200σ, and for the last ten (470th to 480th) 359σ. In most of the experiments a rhythm, similar to that found by Lombard for finger contractions, was noted. In general the author finds that fatigue tends to make work less rapid, less accurate, and highly irregular.

6. Herr Friedrich made his tests upon his class of children, their average age being 10 years. Accuracy of adding and of copying from

dictation was determined under the following conditions: *a*, before the first school hour; *b*, after the first hour; *c*, after the second hour with a rest of eight minutes between the two hours; *d*, after the second hour, no rest; *e*, after the third hour, rests of fifteen minutes between the hours; *f*, after the third hour, one rest of fifteen minutes between the second and third; *g*, after the third hour, no rests; *h*, before first afternoon hour; *j*, after first hour; *k*, after second hour with fifteen minutes between first and second; *l*, after second hour, no rest. The rests were filled with breathing exercises, etc. The results show an increase of errors from *a* to *g* and from *h* to *l*; for dictation experiments this amounted to 370% and 380%, respectively; for the adding series there was, respectively, 103% and 27% increase. It should be noted that only one test under each condition was made; conclusions from the work will consequently only be valid when confirmed by others. The article is important, however, as showing what elementary and secondary school teachers could do for the cause of scientific psychology.

These five studies are an advance beyond ordinary observation. It is slight to be sure, but enough to show the importance, practical and theoretical, of the problem, and to indicate what may be done and what should be done.

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CUTANEOUS SENSATION.

Localization of Cutaneous Impressions by Arm Movement without Pressure on the Skin. C. T. PARRISH. *The American Journal of Psychology*, VIII., 250-267.

Miss Parrish's experiments had a double purpose: first, to test the accuracy with which an observer can indicate, by a pencil held just off the skin, a point previously touched by the experimenter; and, second, to note the effect of trying to emphasize or to exclude visual images in performing this act of localization. Her work is thus closely connected with experiments already reported by Dr. Pillsbury and Miss Washburn.

The results show that in the absence of sensations of contact from the observer's pencil the error in localization is greater than when exploration of the skin is permitted. In those series where especial emphasis was laid on visualization, two of the four observers made

smaller errors than when left to their own native freedom. The error in the case of all four observers reached its maximum, on the other hand, when they were charged to shut out, as far as possible, all visual images. The most accurate localizations were obtained by allowing the observers to see the stimulated point whose position they had subsequently to point out.

As to the *direction* of the error in localization, three of the observers inclined to indicate points too far to the left on both right and left arms; for which the author tentatively suggests the asymmetry of function of the two arms as the explanation. A more constant and striking error in direction, however, was that the point indicated by the observer usually lay nearer the wrist than the point actually stimulated; and the error kept this constant direction both when the arm with which the localization was indicated moved from an extended position and when it started from a position of flexion. The author, in substantial agreement with Dr. Pillsbury, explains this 'peripheral displacement' by a tendency to overestimate the extent of the flexion movements of the indicating arm, and to underestimate that of its extension movements. This is perhaps the true explanation. And yet, since the region of skin experimented on, both here and in Dr. Pillsbury's work, was just above the wrist, the constant direction of error may have been due to the direction of the nearest important basis of longitudinal orientation (that is, the wrist, or, less immediately, the fingers) and not primarily to a false estimate of the movements of the opposite arm. If it can be shown that the direction of error is quite independent of this matter of orientation, brought out so prominently in M. Henri's experiments, the explanation given in the paper will seem much more conclusive. But a passing doubt like this must not be allowed to conceal in the least the value and interest of the results Professor Parrish has given us.

Ueber die Wahrnehmung zweier Punkte mittelst des Tastsinnes, mit Rücksicht auf die Frage der Uebung und die Entstehung der Vexirfehler. GUY A. TAWNEY. Philosophische Studien, XIII., 163-221. Also in Princeton Contributions to Psychology, II., 1, April, 1897.

It has been known that practice usually brings a marked reduction in the threshold distance at which two points on the skin are felt as two; but it has never been quite clear whether repeated experiment on some single selected spot of skin causes a decrease in the threshold all over the body, or whether the decrease is only for the selected spot

and for the one corresponding to it on the opposite side of the body. Volkmann, for instance, believed he had experimental evidence that in repeated determinations of the threshold for some one region of skin the threshold was reduced only over so much of the body as was supplied from closely connected sensory fibres, including the corresponding region on the opposite side. Professor Tawney, on the contrary, has here shown by extended experiments that practice in such a case not merely has a local effect, but lowers the threshold irregularly over the whole body. The changes which we designate as the result of 'practice' are therefore central and psychical.

His farther contribution is in making clearer the exact nature of such practice as is really effective. The practice which causes the threshold to decline is not the mere *repetition* of the discriminative act; for, as Professor Tawney here shows, there may be indefinite repetition of the act, without any reduction of the threshold whatever. If the observer preserves, as far as possible, a calmly receptive attitude toward the stimulus, and allows his judgment to be formed spontaneously as a ready characterization of the external fact, then the threshold remains fairly constant, however often the experiment be repeated. The threshold seems to be reduced by practice only when the observer expects and strains for greater and greater nicety of discrimination as the experiment proceeds. In other words, some form of *suggestion* is the main factor in producing in this field the results hitherto vaguely ascribed to practice. Where suggestion was most carefully excluded, practice had little or no influence on the results. At the same time, the author warns us not to suppose that suggestion is absent merely because the observer has been kept in the dark as to the purpose or method of the experiment. Autosuggestion is at least as pervasive and disturbing an influence as is any other form of suggestion.

Another important feature of the experiments here reported is the intimate connection they seem to reveal between the reduction of the threshold and the puzzling phenomenon of illusory double contact (*Vexirfehler*), so exasperating to workers in this field. The observers were comparatively free from this illusion until practice had considerably reduced the threshold. And in those series where no reduction normally took place, the introduction, at the close of the series, of the suggestion which had been operative in the other cases was the signal both for a sudden drop in the threshold-value and for the appearance of *Vexirfehler*. Dr. Tawney's explanation is that the observer, in his effort to make the finest discrimination between one and

two points, changes the mode of forming his judgment. The observer no longer, as at first, directs his attention to that visual image of the stimulating object which arises in strongest association with the dermal sensation; but now gives his main attention to the dermal sensation itself. The subject's analytic examination of the sensation either accentuates qualitative differences in it, or else produces them outright. And once having definitely before him a distinction within the sensation, it is easy to pass on to the judgment and even to the clear perception of two spatially separate points of contact. But if the observer could rid himself of the preconception that no more than two points were being used, he could often (and one of the subjects actually did) have the illusion of four or five contacts quite as well as of two. By variations in the direction of the suggestion, its influence was brought to light in various ways. However, for these and other interesting details the original paper must be consulted.

Dr. Tawney's account shows that his experiments were carefully arranged and carried out, and his paper is certainly a distinct and important gain for the special field indicated by the title of the article. But, besides this, the principles he touches, in showing the importance of suggestion, have their ramifications in all laboratory work. His admirable article, therefore, can hardly fail to be of assistance in avoiding pitfalls in many lines of psychological experiment other than that to which the author here confined himself.

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Untersuchungen über die Sinnesfunctionen der menschlichen Haut. Erste Abhandlung; Druckempfindung und Schmerz. MAX VON FREY. No. III., xxiii B. d. Abhand. d. math. phys. Classe d. Königl. Sächs. Gesell. d. Wissenschaften. Leipzig, S. Hirzel. 1896.

In this monograph Frey gives an account of experiments on the relations of pressure sensations and their stimuli. His problem was the physiological conditions of pressure stimulation, his method the determination of the threshold relations of time, place, area and intensity.

The first experiments described corroborate the familiar fact that pressure stimuli of a moderate intensity are perceived only a short time after application. Frey concludes that only stimuli near the threshold cause a temporary sensory effect, but in his experiments weights of 100 to 200 g. applied for 1 min. on 100 mm.², failed to cause continu-

ous sensations. The removal of the weight was at times perceived as pressure. This is said by Frey to be due to pressure after-images, but heterogeneous stimuli of low intensity are easily confused. The rapid fading away of the pressure sensation is not necessarily due, as the author assumes, to the fatigue effect of constant stimuli. If the stimulus is not pressure, but the work done on the skin, the stimulus as well as the sensation is but momentary. This is practically admitted by Frey, for in another place he states that deformation of the skin is necessary for the production of pressure sensations.

More important are the experiments on the relations of the threshold to the area, place and rate of application of the stimulus. The apparatus consisted essentially of a lever connected with a balance to which weights were applied and clock work to regulate the rate of increase of the stimulus. Experiments on one observer showed that for him at least the threshold increased at first slowly and then rapidly from .2 to 2 g., as the rate decreased from 6.2 to .7 g. per sec. The data are very meagre, but the results corroborate those which I myself obtained by a less accurate method. It is evident that they support the movement theory of pressure stimulation. The experiments on the area made on two observers seem to show that for low rates of application, 1.2 to 4.3 g. per sec., the threshold increases faster than the area, but for rates of 6 to 11 g. per sec., the relation seemed more a direct proportion. Here, also, the number of experiments is inadequate. More than two constants should be used if even an approximately quantitative relation is to be obtained, especially when the results vary. Frey's conclusion that the intensity of stimulation per unit area varies inversely as the entire area—which he calls the law of 'hydrostatic pressure'—not only contradicts the results of experiments by me, according to which the threshold increases much more slowly than the area, but is also based upon very scant evidence. Curiously enough Frey misuses the term hydrostatic pressure, for liquid pressure increases with the area of application. The experiments on the place of stimulation were made on but one observer. The marked variation in the results for contiguous areas is ascribed to the varying distribution of pressure spots.

The experiments on the threshold made with hairs of known cross-section are of much interest. The law of 'hydrostatic pressure' was found not to hold for areas less than $\frac{1}{4}$ mm². The stimuli seemed to be equal when the pressure increased approximately in proportion not to the superficial, but to the linear magnitude. This proves, according to the author, that the organs of pressure sensation are not on

the surface, and that they are in all probability the corpuscles of Meissner. This interpretation of the experiments was justified by an experiment on a physical model, which showed that under conditions somewhat similar to those of pressure stimulation the pressure exerted on the surface was not fully transmitted below the surface. With these hairs Frey made maps of the pressure points on the calf of the leg and the wrist. The threshold values run from $\frac{1}{2}$ to 4 g. per mm. The average values were the same for these places as for the ball of the thumb and finger tips.

Experiments on the pain threshold were also made with hairs or cactus needles, one of which was affixed to a spring, the whole forming a delicate algometer. The values found for three observers vary from 25 to 50 g. per mm². The 'hydrostatic' law of pressure and area of stimulation was found to hold for all the areas investigated, less than 12 mm². Hence, Frey concludes, the organs of pain are superficial. Their high threshold is explained by the rigidity of the epidermis. The topography of the pain spots was also studied, but apparently the reagent himself applied the stimulus. In these, as in other experiments, especially those on the topography of pressure spots, the author seems to have devoted his attention to physical and physiological rather than to psychological sources of error.

HAROLD GRIFFING.

NEW YORK.

NEW BOOKS.

Man's Place in the Cosmos and Other Essays. ANDREW SETH. New York, Scribners. 1897. Pp. viii+308. \$2.

The Chances of Death and Other Studies in Evolution. KARL PEARSON. With illustrations. Two vols. London and New York, Ed. Arnold. 1897. Pp. ix+388 and 460. \$8.

Émile Zola: enquête médico-psychologique. I. Introduction Générale. ED. TOULOUSE. Paris, Société d'Éditions Scientifiques. 1896. Pp. xiv+285. Fr. 3.50.

Fourteenth and Fifteenth Annual Reports of the Bureau of Ethnology (1892-3, and 1893-4). J. W. POWELL. Washington, Gov. Print. Office. 1896 and 1897. Two vols, pp. lxi+1136; and one vol. pp. cxxi+366.

Congrès international d'Anthropologie criminelle: Comptes Rendus de la IV^e Session (Genève, 1896). Genève, George & Co. 1897. Pp. xxix+396.

- La Structure du Protoplasma et les Théories sur l'Hérédité et les grands Problèmes de la Biologie générale.* YVES DELAGE. Paris, Reinwald & Cie. 1895. Pp. xiv+878. Fr. 24.
- L'Année Biologique: Comptes Rendues annuels des travaux de Biologie générale (Première Année, 1895).* YVES DELAGE. Paris, Reinwald & Cie. 1897. Pp. xlv+732.
- Collezionismo e impulsi collezionistici.* S. DE SANCTIS. Roma, Tip. Innocenzo Ortero. 1897. Pp. 30.
- Sulla dignità morfologica dei segni detti 'Degenerativi.'* V. GIUFFRIDA-RUGGERI. Roma, Loescher & Co. 1897. Pp. 117.
- Appearance and Reality.* F. H. BRADLEY. Second edition (revised), with an Appendix. London, Sonnenschein; New York, Macmillans. 1897. Pp. xxiv+628. \$2.75.

NOTES.

It is with regret that we record the death of Professor W. Preyer, the distinguished psychologist and physiologist, at Wiesbaden, on July 15th; and also that of Daniel Greenleaf Thompson, author of 'A System of Psychology,' etc., in New York, on June 10th.

MR. MUIR, now of Halifax University, has been appointed to the chair of psychology, and Miss Ethel Muir, Ph. D. (Cornell), assistant in philosophy in Mount Holyoke College.

It is expected that the laboratory for Experimental Psychology in University College, London, will be opened in October under the direction of Dr. W. H. R. Rivers, of Cambridge.

A UNIVERSITY Lectureship in Experimental Psychology has now been voted by the Senate of Cambridge University, England.

MR. C. L. HERRICK, lately professor of biology at Denison University, has been elected President of the Territorial University at Albuquerque, New Mexico.

MR. H. P. HYLAN has been appointed instructor in psychology in the University of Illinois.

DR. A. R. HILL has been appointed professor of psychology and ethics in the University of Nebraska, and Dr. E. L. Hinman has been promoted to an adjunct professorship of philosophy in the same University.

F. D. SHERMAN, Ph.D. (Leipzig), has been given the position in psychology and pedagogy in the Oshkosh (Wis.) Normal School vacant by the removal of Dr. Hill to the University of Nebraska.

DR. SIDNEY E. MEZES has been promoted from adjunct to associate professor of philosophy at the University of Texas.

A *Rivista quindicinale di Psicologia, Psichiatria, Neuropatologia* has been inaugurated under the 'Direction' of Professor E. Sciamanna and Professor G. Sergi, with a board of editors of whom Dr. Santo De Sanctis is editor-in-chief, Via Penitenzieri, 13, Rome.

AN *American Journal of Physiology* will be published after the first of January next under the auspices of the American Physiological Society. And a semi-monthly organ of *Zoölogie, Botanique, Physiologie et Psychologie*, called *L'Intermédiaire des Biologistes*, is to be issued by Schleicher Frères, Paris, with M. Alf. Binet as director-in-chief.

NEW volumes in the Contemporary Science Series, edited by Mr. Havelock Ellis and published in England by Walter Scott and in America by Charles Scribner's Sons, will include 'The New Psychology,' by Dr. E. W. Scripture; 'Psychology of the Emotions,' by Professor Th. Ribot; and 'Hallucinations and Illusions,' by Mr. E. Parrish.

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